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THE
EFFECT OF NAUHEIM BATHS UPON
CARDIAC CONDUCTIVITY AND
CONTRACTILITY

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THE EFFECT OF NAUHEIM BATHS UPON CARDIAC CONDUCTIVITY AND CONTRACTILITY.

It will be impossible for many years to come for English patients suffering from the various forms of cardiac and circulatory diseases which are benefited by the Nauheim treatment to go to Germany for a course of baths, and those who have visited that spa on one or more occasions and have derived great benefit thereby will, no doubt, consult their medical advisers as to the possibility of obtaining the treatment in this country. Under these circumstances it seems an opportune moment to draw attention to some of the results obtained from the application of the Nauheim methods in England, and to emphasise the fact that the treatment can be given in London or elsewhere quite as satisfactorily as in Germany.

The administration of the proper form of Nauheim bath in a suitable case produces a slowing and strengthening of the heart's action, a stimulation of the cardiac conductivity, and a definite lowering of blood pressure. Exactly similar results are obtained by the administration of a similar bath prepared artificially.

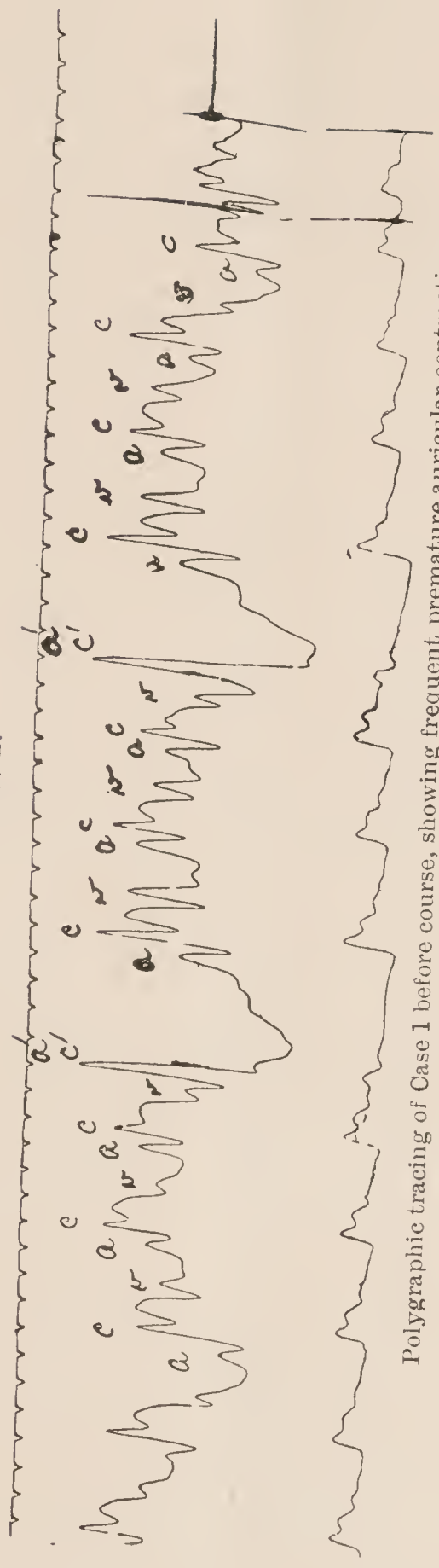
Careful investigations carried out with the aid of auscultation, percussion, and the sphygmomanometer have proved that in patients suffering from dilated enfeebled hearts, the result of myocardial or endocardial disease, the effects produced by a properly administered course of Nauheim baths are a toning up of the cardiac muscle, a greatly strengthened contractility, and a markedly reduced arterial tension in cases where hypertension is present, but has not advanced to that stage in which the vessels have so degenerated as to become practically rigid.

For some time past I have been carrying out investigations with regard to the effect of these baths upon defective cardiac conductivity. Research in this direction was impossible before the invention of the polygraph, and it is to Sir James Mackenzie, the inventor of this ingenious and valuable instrument, that we are indebted for a means of obtaining diagrammatic and accurate records of the varying conditions of cardiac conductivity, and of being able to observe the effects that are produced upon them by the administration of drugs and other forms of treatment.

The following cases with their respective polygraphic tracings are taken from a large number I have treated with the Nauheim methods in London, in all of which I have found defective conductivity to be distinctly benefited in conjunction with the general improvement in health which resulted from the treatment.

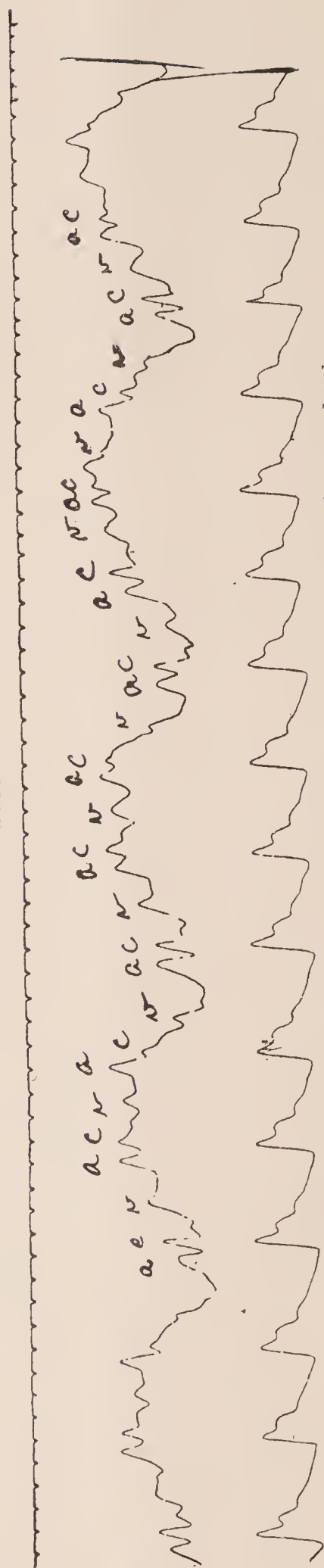
CASE 1.—The patient, a female aged 59, had had an attack of influenza a year before I saw her, since which time she had suffered from pains over the region of the heart on the slightest exertion, dyspnoea, which was worse at night and caused her to spend sleepless nights sitting up in bed, severe palpitation, and much distress from cardiac irregularity. She was unable to do anything, and had for months led an invalid life. When I first saw her her lips were cyanosed and she suffered from marked dyspnoea even whilst sitting in a chair. Her pulse was 84 per minute, and exhibited constant intermittences, which proved to be due to premature auricular contractions; it was small in volume, the tension was greatly increased, being 125–235 mm. Hg, and the vessels were tortuous and thickened. There was marked pulsation in the neck; the impulse at the apex was very forcible and diffuse, and the apex beat was $1\frac{1}{2}$ inches to the left of the left nipple. The area of cardiac dullness measured $7\frac{1}{2}$ inches across at the nipple level, extending from 2 inches to the right of the middle line to 1 inch to the left of the left nipple. The heart sounds were accentuated and a loud systolic murmur was heard over the aortic area. The legs and ankles were somewhat oedematous. The urine was of specific gravity 1015, but contained no albumin, sugar, or casts. As the patient had had treatment by rest and drugs for many months and was not improving in health, I advised her to take a course of Nauheim baths and took her into a nursing home for this purpose, as she was too ill to have it in her own home. Fig. 1 is a polygraphic tracing taken before the first bath; it shows two examples

FIG. 1.



Polygraphic tracing of Case 1 before course, showing frequent premature auricular contractions.

FIG. 2.

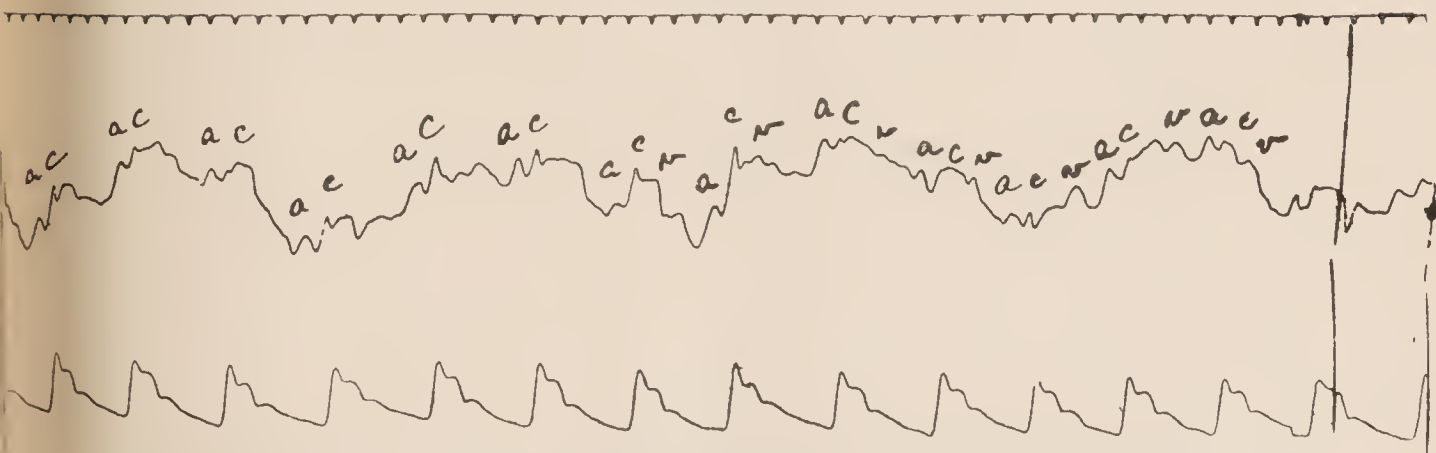


Polygraphic tracing of Case 1 after first bath, showing regular pulse of improved volume.

of the frequent premature auricular contractions. The pulse-rate is 84 per minute and the volume of the pulse rather small. Fig. 2 is a polygraphic tracing taken ten minutes after the first bath; it shows a regular pulse of improved volume and slower rate, 58 per minute. The improved conductivity is demonstrated by the absence of premature auricular contractions.

After a course of 25 baths extending over five weeks the patient was in a much better state of health. She had no dyspnœa and could sleep well at night without any pain or palpitation; there was no œdema of the legs or ankles, and the blood pressure had fallen to 120–200 mm. Hg, a drop of

FIG. 3.



Polygraphic tracing of Case 1 after course, showing regular pulse of good volume; rate 68.

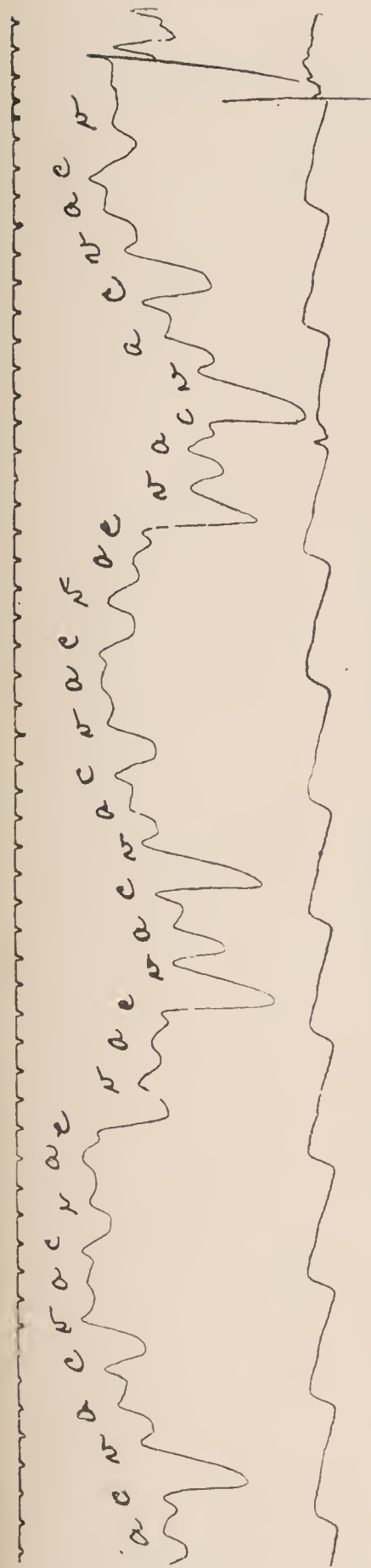
35 mm. Hg in the maximum pressure. The area of cardiac dullness measured 5 inches across at the nipple level and extended from $1\frac{1}{4}$ inches on the right of the middle line to $\frac{1}{2}$ inch inside the left nipple; the apex beat was in the nipple line; the cardiac impulse was much less forcible and diffuse. Fig. 3 is a polygraphic tracing taken at the end of the course and shows a regular pulse of fair volume and 68 per minute. Nine months after the treatment this patient was still enjoying good health and her pulse was quite regular.

CASE 2.—The patient, a female, 58 years of age, consulted me on account of great stoutness, dyspnœa, bronchial catarrh, palpitation, and increasing œdema of the legs. She was somewhat cyanosed, markedly obese, weighing 17 st., and being 5 ft. 10 in. in height; her dyspnœa, even on the slightest exertion, was marked, and she could not walk except at a slow pace and for a short distance. The legs were decidedly œdematous up to the knees, pitting fairly deeply on

pressure. Her pulse in the upright position was 96 per minute, regular in time and volume, and of small volume; her blood pressure was 75-150 mm. Hg. Her area of cardiac dullness was much enlarged, extending from 2 inches outside the left nipple line to 3 inches to the right of the mid-sternal line; the cardiac sounds were heard very faintly at the apex, but were not audible over the base of the heart. There was no albumin or sugar in the urine. The polygraphic tracing, Fig. 4, showed in the venous curve an *a-c* interval slightly *longer* than the normal, and in the radial curve a regular pulse of very small volume, and 70 per minute in the recumbent position. This patient had a course of 25 baths extending over a period of five weeks. At the conclusion of the treatment her weight was 15 st. 12 lb., having dropped 1 st. 2 lb. in the five weeks. She was of a good colour, the dyspnoea was decidedly less marked, and she could take fairly lengthy walks of one or two miles extent. There was no oedema of the legs, and the bronchial catarrh was much less troublesome. The pulse was 90 in the erect and 68 in the recumbent position. The area of cardiac dullness was markedly less, extending from 3 inches inside the left nipple line to the mid-sternal line. The position of the breast, which was large, had somewhat altered by the loss of weight, so that the nipple was not exactly in the same position, and the area of cardiac dullness had not decreased quite so much on the left as would appear from the measurements. The area of cardiac dullness was, however, about normal. The cardiac sounds were much stronger and could be well heard both at the apex and over the base of the heart; no murmur was audible at any time during treatment. The blood pressure was 80-130 mm. Hg, having fallen 20 mm. since the commencement of the treatment. Fig. 5, a polygraphic tracing taken at the end of the course, shows in the venous curve an *a-c* interval slightly *below* the normal, and in the radial curve a pulse of much improved volume and slower rate. The symmetry of the tracing is spoilt by the pens meeting at times, but the central portion is clear.

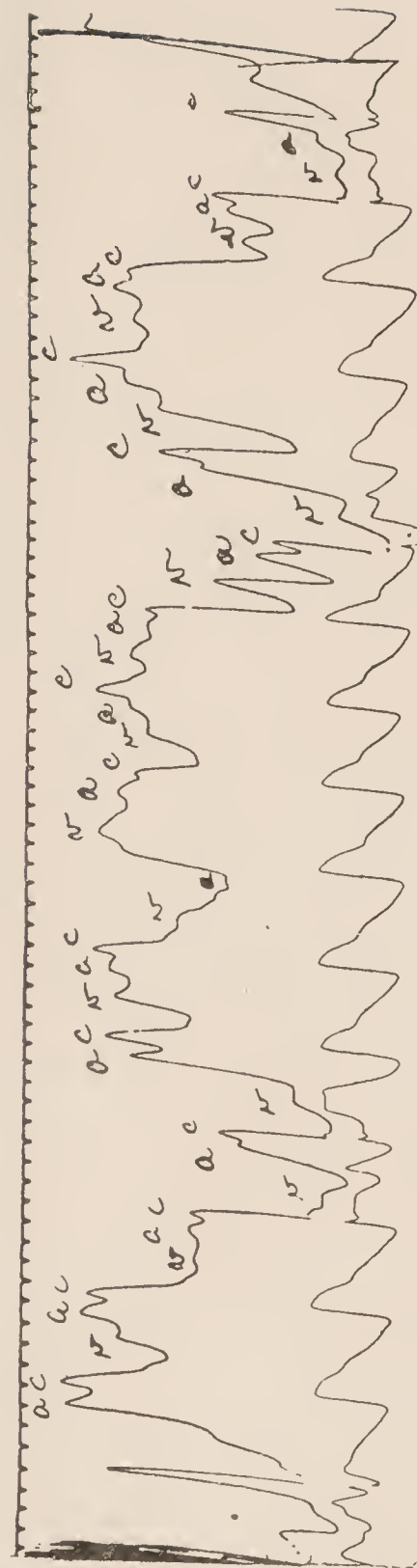
The above two cases show the typical improvement obtained in cardiac conductivity and contractility by a course of Nauheim baths administered in London. Clinically they differ widely from each other and are thus illustrative of the fact that in the majority of cases suitability for the Nauheim treatment is rather a question of severity of the disease than of kind, though there are exceptions to this rule. The case of a dilated enfeebled heart in a patient with high blood pressure is *a priori* the one that gives the best results, and the fact that

FIG. 4.



Polygraphic tracing of Case 2 before treatment. The α - c interval is slightly longer than the normal.
Pulse 70 per minute in recumbent position.

FIG. 5.



Polygraphic tracing of Case 2 after treatment. Pulse 68 per minute. The α - c interval is slightly less than normal.

valvular disease is also present is no bar to the treatment, though the valvular defect itself cannot be cured.

In Case 1 the effect of the baths upon the defective cardiac conductivity was very marked because the defect was very material. In Case 2, though the decrease in the *a-c* interval produced by the baths was only small, it must be remembered that that interval is normally only 1/5th of a second in duration, and that an increase of 1/15th to 1/20th of a second indicates a marked slowing of the conductivity.

In a large number of cases of cardiac and circulatory impairment the only indication of a defective conductivity is a slight lengthening of the *a-c* interval. When sufficient time has passed to allow observers to follow up such cases over long periods, I believe it will be found that this slight impairment is the first indication of a condition which, if untreated, leads ultimately to serious defects in the nervo-muscular mechanism of cardiac conductivity. At present this theory cannot be proved, but we are able to demonstrate the great improvement in the condition of the patient after the conductivity and contractility of the heart have been brought into a more normal condition by suitable treatment.

Harley-street, W.

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MODERN ABDOMINAL SURGERY

THE BRADSHAW LECTURE

DELIVERED AT THE

ROYAL COLLEGE OF SURGEONS OF ENGLAND

DECEMBER 18TH, 1890

WITH AN APPENDIX ON THE CASTRATION OF WOMEN

BY

SIR T. SPENCER WELLS, BART., F.R.C.S.

SURGEON TO THE QUEEN'S HOUSEHOLD



LONDON

J. & A. CHURCHILL

11 NEW BURLINGTON STREET

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TO THE
PRESIDENT, VICE-PRESIDENTS, AND COUNCIL
OF THE
ROYAL COLLEGE OF SURGEONS OF ENGLAND
THIS LECTURE
DELIVERED AND PUBLISHED BY THEIR DESIRE
WITH AN APPENDIX ON
THE CASTRATION OF WOMEN
IS RESPECTFULLY DEDICATED

MODERN ABDOMINAL SURGERY

MR. PRESIDENT AND GENTLEMEN,—Some who hear me to-day—I fear not many—may remember the condition of Abdominal Surgery in the early part of the Victorian age, forty or fifty years ago. Younger men may easily inform themselves on the subject by referring to the surgical dictionaries and text-books of the period; and all must acknowledge that the contrast with the Abdominal Surgery of our own time—with what we may call Modern Abdominal Surgery—is very remarkable. An occasional operation for strangulated hernia was almost the only piece of abdominal surgical work done in those days. The radical cure of hernia had scarcely begun to attract attention. Astley Cooper had tied the abdominal aorta, and a case of Cæsarean section, when it was heard of, was talked of by the profession and the public as a marvel. A gunshot or other penetrating wound of the abdomen was rarely met with in civil practice; and there, with the occasional formation of an artificial anus, Abdominal Surgery may be said to have reached its boundaries; for neither then nor now have operations on the rectum, nor removal of stone from the bladder until quite recently, been classed as parts of Abdominal Surgery. Take Syme's 'Principles of Surgery' as an example. In the fourth edition, published in 1856 (a book which he says in his preface is the result of thirty years' hospital experience, and has been tried by a long succession of pupils at home and abroad), the surgery of the abdomen is treated in twenty-eight pages, four of which are devoted to wounds; penetrating wounds, he says, being almost certainly fatal. One page given to tapping, twelve to hernia, and a few lines on iliac abscess complete Syme's survey of Abdominal Surgery in 1856. Compare this with the recent

work of an English provincial surgeon, Mr. Greig Smith, surgeon to the Bristol Infirmary. In the second edition, published in 1888, we find forty-six pages on the diagnosis of abdominal tumours, forty on abdominal operations generally and their after-treatment, sixty on ovariectomy, ten on the Fallopian tubes and broad ligaments, thirty on operations on the non-gravid, and sixty on the gravid uterus; 140 on operations on the stomach and intestines, fifty on the kidneys, forty on the liver and gall-bladder, and twelve on the spleen and pancreas. Then we have a few pages on omental and mesenteric tumours and intra-peritoneal cysts. A long chapter on suprapubic cystotomy follows, and then some sixty pages are devoted to wounds and injuries of the hollow and solid viscera, perforating ulcers, purulent collections, and tubercular peritonitis. It is difficult to imagine a more striking contrast than this of abdominal surgery as it was forty years ago and is now, or to contemplate without surprise the vast and rapid advance made in our own day and generation, first in this country and afterwards abroad. In systematic works for students, and books of reference for practitioners, the sections on abdominal surgery are much enlarged. You, Mr. President, were one of the earliest of the leaders in this advance. The successive editions of your own 'Practice of Surgery,' like those of Erichsen's 'Science and Art of Surgery,' confirm all that I have said, and a comparison of 'Heath's Dictionary' with that of Samuel Cooper would do so quite as strongly. We have the well-known works of Treves on 'Intestinal Obstruction' and Morris on the 'Surgery of the Kidneys;' and I am glad to be able to say that the subject has not been neglected in this theatre. Mr. Treves's Hunterian Lectures in 1885, on the Anatomy of the Intestinal Canal and Peritoneum in Man, mark a distinct advance in our knowledge, and improvements in our practice.

In 1878, as Hunterian Professor, I delivered six lectures in this College on the 'Diagnosis and Surgical Treatment of Abdominal Tumours.' Two of those lectures were devoted to the Diagnosis, and four to the Surgical Treatment of such tumours. Three were restricted to the treatment of Ovarian Cysts and Tumours, especially to Ovariectomy, and to the consideration of Antiseptics in Abdominal Surgery. The Surgical Treatment of Uterine Tumours was the subject of the concluding lecture. It was

based upon the whole of my experience up to that time. A short description of Freund's method of entirely removing a cancerous uterus by abdominal section completed the survey that I was able to take of the state of Abdominal Surgery twelve years ago. Ten years later—in 1888—in the Morton Lecture on Cancer, I entered more fully into a description of the mode of extirpating the entire cancerous uterus by the vaginal operation.

Since 1878 the development of Abdominal or Peritoneal Surgery has been wide and rapid. When, in 1885, I published, in a condensed form, a small book on the 'Diagnosis and Surgical Treatment of Abdominal Tumours'—which might be called a fourth edition of that published in 1865 on 'Diseases of the Ovaries'—I had to describe the wide spread of the domain of Abdominal Surgery; to make many additions which naturally arose out of the growth of the subject, and to include the operative treatment of various kinds of tumours—splenic, renal, hepatic, mesenteric—and describe other operations hardly noticed in the earlier editions.

No intelligent student of the history of our science and art can doubt that ovariectomy was the starting-point in the modern advance of Abdominal Surgery. The first extension was to uterine tumours, and to partial and complete extirpation of the uterus. Although I have formerly alluded to these subjects in the Hunterian and Morton Lectures, experience has accumulated so rapidly of late years, that I may perhaps offer for your consideration a few remarks suggested by later modifications of these uterine operations, and upon some other of the more recent developments of Abdominal Surgery. But I will first allude to some practical questions which are still waiting for a decided answer, and which apply to nearly all surgical operations.

ANÆSTHESIA

The first question is, Which is the safest and best anæsthetic? Is it chloroform, or ether, or a combination of the two, or the mixture of alcohol, chloroform, and ether known as the A C E mixture, or bichloride of methylene, or laughing gas, or anything else? Beyond all doubt chloroform is still the usual and favourite anæsthetic. But I was from the first afraid of it.

The only death I ever witnessed of a patient under an anæsthetic was from chloroform. This was in 1848, and the surgeon was Malgaigne. The first year I was at the Samaritan Hospital, in 1854, I amputated a small breast, and the patient very nearly died from the chloroform. For a time we thought she was dead, and it was only after prolonged artificial respiration that she recovered. In several of my earlier cases of ovariectomy I was very uneasy about the effects of the chloroform during the operation, and in more about the vomiting which I thought it set up after operation; and twice, when Clover administered from his bag the vapour diluted with air, I had to stop my work while a patient was resuscitated. Whether chloroform was given by lint and a towel, or by Skinner's mask, or by some inhaler, I was always much more anxious about the anæsthetic than about hæmorrhage or any other operative detail; so that when, in 1867, Dr. Richardson explained his views as to the causes of danger of death from chloroform, and his belief in the greater safety of methylene, which he was then introducing, I was quite prepared to give the newer liquid a fair trial.

To my mind, the result of the first case was most satisfactory, and I have repeatedly made known what my experience of methylene has been. I have been surprised that, in the face of the reports of deaths from chloroform repeated week after week in the newspapers and Medical Journals, we have not yet had to defend one of our brethren against a verdict of manslaughter on the ground that an anæsthetic, well known to be dangerous, had been administered when others, equally efficacious, were known to be safer. I should not at all like to be tried on such an issue, for I fear the defence would be very difficult. I am sorry I cannot enter more fully into this question, but there are others which demand more time than I have at my disposal, and I must be content with explaining that some of the reasons urged against the use of methylene may be completely answered. It can be made by any manufacturing chemist in the manner described in his first paper by Dr. Richardson. Its chemical composition shows it to differ from chloroform only in containing one equivalent less of chlorine.

	Composition	Specific gravity	Boiling-point
Bichloride of Methylene	CH_3Cl_2	1320	128°
Chloroform	CH_3Cl_3	1480	142°

It is not so easy to procure pure methylene as pure chloroform; for, in spite of the greatest care, a little chloroform, from which methylene is reduced by the action of zinc, occasionally passes over during distillation; but the quantity is too small to be of much consequence. Still I trust the makers will be able to guard against this accidental admixture. Even as now sold, if it is administered sufficiently diluted with air, as it may be from Junker's inhaler by any intelligent student, or even, in cases of emergency, by a nurse, I believe any surgeon who will try it on my recommendation (after more than twenty years' experience of its use in a very large number of operations, some of them exceptionally long and trying), will be freed from much unnecessary anxiety, and may escape censure which some might think to be not quite undeserved. I am sorry I cannot devote more time to this important discussion now; but I have some reason to believe that the whole subject may be treated fully, either in this theatre or in the Examination Hall of the two Colleges, in a full course of Lectures on Anæsthesia by Dr. Richardson, including all the substances described in his synopsis of anæsthetics in the second volume of the 'Asclepiad.'

Let me now pass on and ask you to consider for a few minutes the question of

DRAINAGE,

which, with or without *flushing* of the peritoneal cavity, is one of the more recent additions to the practice of Abdominal Surgery. First introduced and practised by Peaslee in 1855, rather in the treatment of septic peritonitis *after* ovariectomy than as one of the steps of the operation, it has been followed extensively in America, in this country, and in Germany. Some surgeons attach great importance to it, and adopt it almost as a general rule, even where there has been no escape of fluid or oozing of blood into the peritoneal cavity. Koeberlé and Keith first used glass tubes $\frac{1}{4}$ to $\frac{1}{2}$ an inch in diameter. Since then smaller tubes of vulcanite have been preferred, and various modes of syringing, attaching waterproof protectors or sponges, have been used to carry off fluid and to prevent the entrance of septic matter into the cavity. In my own work, I have from the first looked upon drainage as a practice to be avoided if possible, and have only put in a tube when I knew I had not been

able to cleanse the peritoneum thoroughly, or thought that some oozing was likely to go on after the incision was closed, or when, some days later, I had reason to suspect the presence of fluid in the cavity. But I soon began to think that the tube acted as an irritant and led to the formation of the fluid which it served to remove. At first, when I was in doubt, I put in a tube. But very soon, when in doubt, I left it alone. More than once I was sorry I had not used it, but much more often I was glad; and so early as 1876, in a paper read before the Royal Medical and Chirurgical Society on completing 800 cases of ovariectomy, I argued that drainage should only be an exceptional practice. Later, in 1885, after an experience of more than 300 additional cases, I maintained that it should be 'almost entirely discarded,' and said, 'I have not drained one case in which antiseptic precautions have been taken; and, on looking back, I cannot believe that there are more than two or three in which, if a drainage-tube had been used, it would have been useful. The simple explanation is, that the mixture of blood, other fluids, and air, left in the peritoneal cavity, or oozing into it after operation, formerly went through putrefactive changes, and, if not drained off, produced septicæmia, whereas now no putrefaction takes place, and absorption is quite harmless.' ('Abdominal Tumours,' page 61.)

This was six years ago. I can now add that I have only twice *flushed* or washed out the peritoneal cavity with warm water, and in both cases I regretted having done so. When the bladder or intestine has been wounded by gun-shot or otherwise injured, and urine or fæcal matter has escaped into the abdominal cavity; or when pus has escaped from an abscess, flushing may secure more complete cleansing than simple sponging, and so become really valuable; but in a large majority of cases of removal of abdominal tumours, it simply adds to the amount of sponging required at the expense of more or less shock or depressing effect, and leaves the patient no better, perhaps worse, than after careful sponging with soft moist sponges. I have tried several substitutes for sponge—soft linen or cotton handkerchiefs, and absorbent cotton enclosed in muslin—but have not yet found anything that answers so well as sponges. I still preserve them from infective pollution in the manner I have repeatedly described.

I am well aware that two of my successors at the Samaritan Hospital drain much oftener than I ever did, and often flush; and they regard both practices as valuable additions to Ovariectomy. Their results are excellent, but I must be guided by my own experience, and it is my duty to let others know what I think I have learned. The question is so important, and still so undecided, that I cannot refrain from relating some cases in my practice of this year, where I hesitated as to flushing and draining. They seemed to be exactly the cases where at least drainage was imperatively necessary, yet they recovered admirably well without either flushing or draining.

Last spring a lady from Yorkshire, aged sixty-three, consulted Dr. Matthews Duncan, whose death we have so recently been deploring. He found an abdominal tumour, was doubtful whether it was uterine or ovarian, feared it might be malignant, and advised postponement of any operative treatment. Let me say a word of respect for the memory of Matthews Duncan. I esteemed him highly, as an able, thoroughly conscientious, and careful physician. I can support all that Mr. Doran has said in his interesting memoir (published in the 'American Journal of Obstetrics') of the great good effected by Duncan by 'instilling high principles of professional morality into the minds of his disciples.' We are all grateful for his valuable addition to our Museum of the many beautiful coloured drawings of various diseases classed together as Lupus. I can respect the feeling which led him, in 1857, to publish his paper, 'Is Ovariectomy justifiable?' and I cannot deny that the opinion he then expressed was justified by the facts then on record. His deliberate conclusion was that the defenders of Ovariectomy 'have nothing but flimsy and fallacious arguments' to offer in its support. I frequently met Duncan in friendly consultation, and have not infrequently argued with him that, like some other physicians, his advice tended to postpone surgical operations until neither patient nor surgeon had a fair chance. The physician often thinks the surgeon rash or venturesome, inclined to operate before the necessity for operating is sufficiently proved, and too sceptical of the good effect of expectant or medicinal treatment. The surgeon quite as often believes that if he could operate before the disease has brought the patient into a condition when recovery is doubtful or improbable,

failures in saving life would be fewer, and success more certain and complete.

In the case I now speak of, a delay of three months was the result of the first consultation. Then sudden and rapid increase in the size of the abdomen took place, and the patient again came to London. Dr. Andrew saw her, and, guided by the former history, expressed his fears as to the case being one of peritoneal cancer and ascites. When I saw the lady for the first time I expressed a very confident opinion that there was no evidence of malignant disease; but that there was strong ground for hoping that the free fluid in the peritoneal cavity proceeded from a burst ovarian cyst; that both ovaries were enlarged and the uterus normal. After considerable opposition I was allowed to make an exploratory incision and act upon what I might discover. This I did—removed many pints of ovarian fluid, and both ovaries, after detaching adhesions to the abdominal wall, intestines, and omentum. Then came the question of flushing or drainage. I never saw a case where it appeared to be so necessary. The peritoneum, wherever it could be seen, was soft, red, thickened, covered by loosely adhering pasty layers of lymph, broken-down ovarian structures, and blood-clot. A great deal of sponging left it imperfectly cleansed, and I was thinking of flushing when the patient appeared to be so extremely weak that I was glad to complete the operation as rapidly as possible and get her to bed alive without a drainage-tube. I arranged with Mr. Robert Priestley, who attended to the after-treatment, that if any sign of accumulation of fluid should appear, or much rise of temperature, we might remove a stitch and insert a drainage-tube. But neither of us after the operation had the slightest uneasiness or apprehension. There was neither pain, sickness, nor fever, but uninterrupted recovery. She went into Yorkshire four weeks after the operation, and I saw her in London in October in excellent health.

I had a very similar case last spring—a patient of Mr. Maso of Ross, in Herefordshire, where the operation had also been put off until rupture of a cyst of the right ovary had occurred some three weeks before I operated. The abdominal cavity was filled with ovarian fluid, and the peritoneum everywhere had the most alarming aspect. It was covered all over with flakes of lymph, blood-clot, and masses of proliferating papilloma.

was quite impossible to cleanse it thoroughly, with or without flushing, and I was content with doing all I could by sponging. Twelve or fifteen years ago I should have put in a drainage-tube, and, if the patient had recovered, have probably attributed the recovery to the drainage. Now, as she recovered admirably well, I believe she recovered partly because I did not drain—that the drainage would have exhausted her and might have led to infective or putrefactive processes.

I was much surprised at such rapid and apparently complete recovery, and was not at all surprised to hear that, after a few months at home, she became ill and the abdomen again enlarged. She came to London, and I saw her on October 30. The abdomen was much distended, and the cicatrix of the operation extremely thin. There was free fluid in the peritoneal cavity, and a well-defined tumour in the left iliac region, which could also be felt by vaginal examination. Although I had made no note of the operation in February that the left ovary was of normal size, but more closely attached to the uterus than usual, I had little doubt that it had enlarged rapidly, and that the fluid in the peritoneal cavity was, as before, ovarian. One could scarcely operate under less favourable conditions, but the patient was in a state of extreme suffering, and it seemed wrong to allow her to die without making some effort to save her. Accordingly, after one day's rest, I operated on November 1. The free peritoneal fluid was clear and light-coloured. There were many pints of it, perhaps thrown off from the surface of the peritoneum, which was everywhere covered by, or converted into, a rough, irregular, rather hard layer of granular papilloma. A very thin-walled cysto-sarcomatous mass formed by the left ovary, enlarged to the size of a small adult head, was then removed, and the pedicle tied in the usual way with silk ligatures. I found, quite loose in the peritoneal cavity, a mass as large as an orange, so smooth that it did not look like what I had just broken up, but as if it had escaped from the cyst some time before. This I took out, and other smaller masses were sponged away. I removed all I could by sponging, but did not either flush or drain.

I need not say how much I feared a rapid collection of fluid secreted by the diseased peritoneum, or how surprised I was that nothing of the kind occurred, and that the patient recovered

as well as after the first operation. The temperature remained almost normal; there was no sickness, very little pain, and when I removed the stitches on the seventh day the wound had healed completely, and, so far as anyone could see, recovery was complete. She left London twenty-six days after operation.¹

I went to Paris last July to see an American lady, seventy-five years of age, in consultation with Dr. Faure Miller and Dr. Bouilly. All the facts pointed to recent rupture of an old-standing cyst of the right ovary. There was a great deal of free fluid in the peritoneal cavity, and a large solid tumour, certainly not uterine. The necessity for immediate relief was urgent, and with the kind assistance of Dr. Bouilly, on July 25 I made an exploratory incision. A large pailful of turbid fluid escaped from the peritoneal cavity, and then we found an enormous mass of papilloma. It converted the thickened omentum into a large tumour, and covered the peritoneum of the abdomen and pelvis with such a dense layer of sprouting growth that it was quite impossible to ascertain or do anything more. I had scarcely a doubt that drainage would have been a fatal mistake, and closed the wound. I heard that Dr. Bouilly removed the sutures on August 1, and 'found everything healed, and says she could not have done better had she been twenty years of age.' I had a very satisfactory report of her condition a few days ago. There had been no fluid formed since the operation.²

It is very curious that simply removing fluid by tapping does not appear to have any such beneficial result as incision, either in cases of tubercular peritonitis, or in cases of papilloma of the peritoneum, whether these have followed bursting of an ovarian cyst or have originated from some other cause.

The fact of the extraordinary recovery of patients whose peritoneum seemed to be in a hopeless condition, and their remaining many years after the incision in good health, was noticed by Mr. Thornton in 1881. I have been greatly surprised at several such cases in my own practice, and Dr. Keith has met with others.

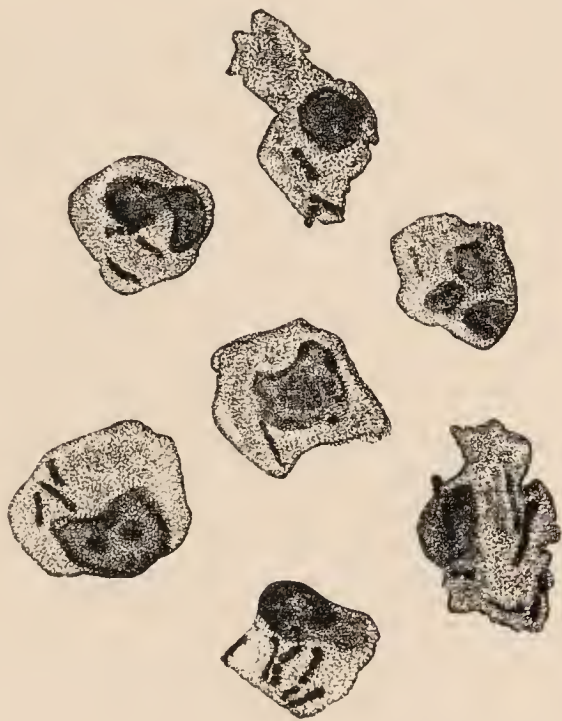
The extremely important practical question of drainage cannot be discussed without some thoughtful consideration of Dr.

¹ January 1891.—I hear from Mr. Cutfield, who succeeded Mr. Mason, that the patient has gone on well since her return home.

² March 1891.—This lady died last month, no fluid having collected.

Ruffer's recent investigations on the destruction of micro-organisms by amœboid cells—on the phagocytes of the alimentary canal—and on the processes which take place in diphtheritic membrane. Koch's most recent bacteriological researches, and Hankin's work on defensive proteids, also assist in unravelling much that has been, and is still, mysterious. More than the whole of one lecture would be required to treat these subjects at all fully, and I must not now attempt to do more than present a bare sketch or outline of what has been done, rather as encouragement for future research than as conclusions for present guidance.

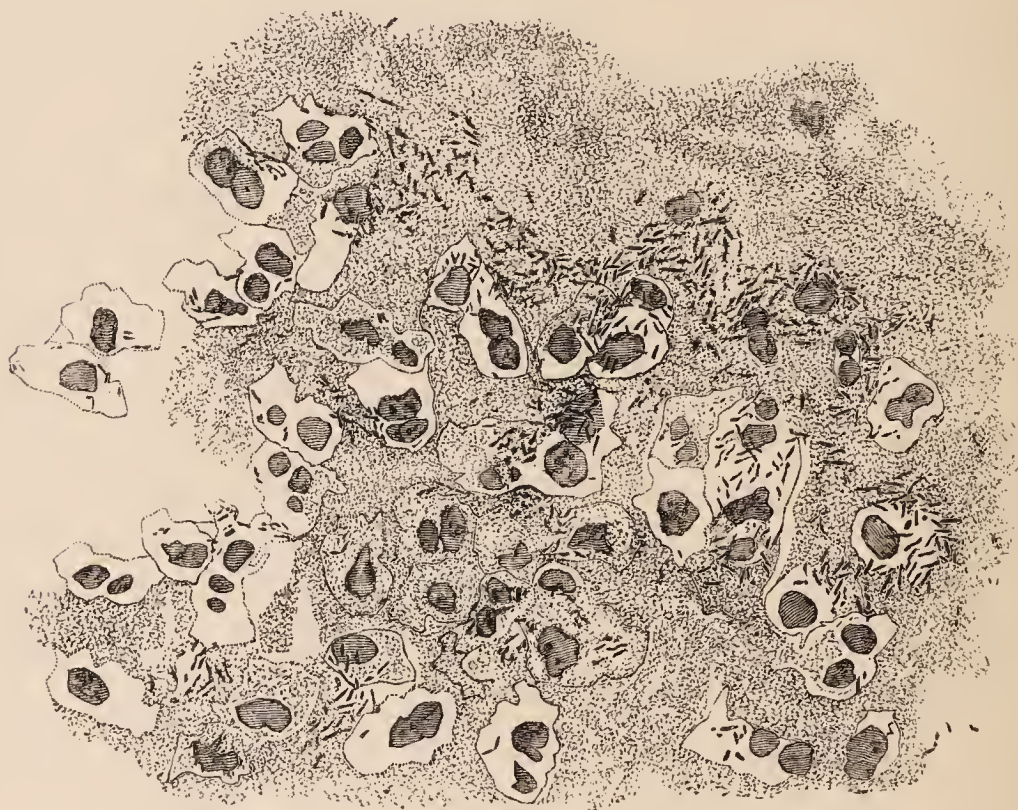
The Phagocyte theory of Metchnikoff, or rather his observations upon the wandering cells or leucocytes by which the animal body protects itself against the attacks of bacteria—taking in the bacilli, digesting them, and so preventing their multiplication and diffusion—explain much that was almost incomprehensible in the relations of bacteria to wounds and to infective diseases. Inflammation is set up in living tissues by the microbes, lymph is effused, and the lymph is soon crowded by phagocytes, which stop the further development of the microbes if they are not present in overwhelming numbers. This battle between the attacking specific microbes and the defending phagocytes has been well demonstrated by Dr. Ruffer in the diphtheritic membrane in man. He has kindly lent me some of his sections, where one or several bacilli may be seen, enclosed in a phagocyte, some of them perfect, some in process of being digested, or disappearing.



We have here a number of preparations which may be seen on the screen showing microbes and phagocytes—the defending cells and the invading enemies. They will be kindly shown by Dr. Woodhead and Mr. Pringle. All I will show now are some of the bacilli of diphtheria and the phagocytes devouring and digesting them. And you will also see some of the very characteristic bacilli of tetanus sent over by M. Pasteur. They

are before you in different stages of development, from the simple rod to the rounded end which shows the sporulation. I may advise all those who are interested in the influence of bacteriology on surgery to wait until after my lecture and see a number of interesting specimens of the infecting agents in a variety of diseases—anthrax, diphtheria, tubercle, tetanus, silk-worm disease, and fowl cholera—which Dr. Woodhead and Mr. Pringle have ready for inspection—the opening up of a revolution in surgical pathology.

In healthy animals, the protecting cells are victorious unless the microbes are extraordinary in numbers or virulence. In



the feeble, the pathogenic organisms, or the soluble poisons they secrete or form, pass into the blood, the spleen, and the liver, and destroy the animal. In the Peyer's patches and tonsils of some animals—of the rabbit, for instance—the struggle, according to Dr. Ruffer, is a physiological process, taking place constantly at every hour of the day.

A new and unexpected support to the phagocyte theory is to be found in Mr. Hankin's recent work on defensive proteids. He has isolated from the spleen and lymphatic glands of various animals a proteid body which has the power of killing bacteria. At the last meeting of the British Association for the Advancement of Science at Leeds, he stated that this defensive proteid,

though absent from normal blood, could be obtained from that of febrile animals. If this assertion be accepted as correct, we must believe that, when menaced by a hostile microbe, the organism attempts to protect itself by throwing into the circulation a substance which has bactericidal powers. This view agrees with Mr. Hankin's suggestion that phagocytes can not only kill the microbes they have taken in, but can also (by liberating their contents) exert a bactericidal action.

The question also arises whether animals which are naturally refractory to some special disease may not be so because they have the power of producing some particularly active variety of defensive proteid. If this should prove to be the case, we may hope that (by isolating such substances) remedies, protective or preventive, against various infective diseases, may be discovered and obtained. It seems pretty clear that what Koch and the German pathologists call 'toxalbumen' may be the pathogenic albumose of Hankin.

My excuse for alluding to these investigations while they are still in progress is, that, taken into consideration with the power which the serum of healthy blood possesses of killing bacteria—a power connected probably with the presence of a globulin which is only soluble in dilute solution of common salt—they may explain the antiseptic action of common salt and strengthen the belief that we have in dilute saline solutions a safe and useful fluid for surgical irrigation, preferable to water which has been sterilised by boiling, and possibly to solutions of phenol or perchloride of mercury, either of which may irritate the tissues of the patient or injure the instruments of the surgeon.

Some such reflections as these have been leading me to the conclusion that the use of antiseptics in modern abdominal surgery may be summed up as combining the utmost possible cleanliness of the body, the clothing and the surroundings of the patient, the surgeons, assistants, and nurses; the perfect sterilisation of all instruments by boiling water, as well as of the silk or any material used for ligatures or sutures; and the absolute purity of the sponges or any substitute for them. I long ago asserted that in our daily work we had much grösser causes of danger to guard against than enemies which are invisible without a microscope—that dried clots of blood, or fragments of diseased tissue, imperfectly cleansed from our knives, scissors,

forceps, or needles, may act as directly as vaccine matter. Careful assistants or nurses may not perform this duty of cleansing perfectly, and I think what we are learning from the bacteriologists leads to the conclusion that it will be well after all operations, and probably before them also, to boil all our metallic instruments for a few minutes. I have here a simple contrivance for doing this without much trouble. The credit of the contrivance is due to Mr. Cathcart, of Edinburgh. One great advantage of it is that the instruments are taken out quite dry, and remain free from rust.

A very useful series of experiments by Staff-Surgeon Macpherson is now being carried on at the laboratory of the Conjoint Colleges, in order to test different methods of keeping silk or other material for sutures and ligatures aseptic. Some of the tubes on the table here, containing sterilised meat infusion and pieces of silk or catgut prepared in various methods, have been kept for some weeks at a favourable temperature for incubation. In some no putrefactive or germinating changes can be observed. In others these changes are clearly manifest, and the general conclusion so far is, that solutions of 10 per cent. of carbolic acid in olive oil are useless, and so is wax impregnated with carbolic acid, while a watery solution of corrosive sublimate, 1 in 2,000, preserves silk ligatures aseptic. I mention this in passing as one instance of the useful work already begun in our laboratory.

I used the spray at first hopefully, then operated alternately with and without it, and then gave it up altogether. As I have said, I regard drainage as a rare, exceptional practice. The dressing may consist mainly of sterilised absorbent cotton. Whether by any attenuated inoculation, or the injection of any defensive proteid, a patient can be still further protected from septicæmia or tetanus, or the microbes by which they are caused can be destroyed after the battle between the enemy and the defence has begun, is a question which I trust some worker in our rooms upstairs, or on the Embankment, may not leave to our Continental brethren without a close race, handicapped though we may be by mistaken interference and unwise restriction.

That this hope is about to be realised I gather from experiments as yet scarcely completed made in Koch's Institute by his

assistant, Dr. Behring, on diphtheria, and on tetanus by Dr. Kitasato, a Japanese. The experiments seem to prove almost certainly that we can prepare one vaccine which confers upon mice, rabbits, and guinea-pigs immunity from tetanus, and another from diphtheria, and which also, after these diseases have begun, will stop further progress and save the creature who would otherwise certainly die. The poisons of tetanus and diphtheria are terribly potent, but admit of accurate dilution to an extent which will either kill an unprotected animal in twenty, forty, or sixty hours, or in four to six days; or will by injection into the peritoneal cavity cure a poisoned animal after tetanic symptoms have begun, or will confer immunity upon these animals, protecting them against the bacilli and their products.

Before I pass on I cannot avoid contrasting Matthews Duncan's conclusion respecting ovariectomy in 1857, a year before my first case, with the present position of the operation. My own completed operations alone amount to 1,230, with 19 additional operations for the second time on the same patient, or 1,249 in all. Several other operators, at home and abroad, can count their cases by the hundred; and it is certain that the immediate results are fully as satisfactory as those of any serious surgical operation. The subsequent history of the patients for several years after operation has been more fully and accurately obtained and recorded than can be said of any equally important operative work. Let me ask, could anyone have imagined that, in such a small hospital as the Samaritan, in little more than thirty years, there would be performed, as there have been, 1,378 cases of ovariectomy, with a mortality of only 14·13 per cent., or that, in the last four years, in 259 cases, there would be only 12 fatal—a mortality of only 4·40, less than 5 in the 100? It is equally remarkable that in successive series of 100 cases, and in successive periods of five years, from the earliest cases until now, progressive improvement has been as steadily maintained in this hospital as in my own practice.

UTERINE TUMOURS

When I lectured here in 1878 on Uterine Tumours, and gave the history of the whole of my practice up to that time of their removal, or attempted or partial removal, very few

surgeons here or abroad had much experience of the operation. Now, it has become, if not as frequent as ovariectomy, still an operation in which not only some British and American, but German and French, surgeons can tabulate their cases by the hundred. And some important practical questions, still undecided, may be discussed with the help of accumulated facts. Perhaps the most remarkable conclusion of any is that of the most successful of all operators who has had a larger experience of these operations than I or any other surgeon has had—Dr. Keith. His results are magnificent; yet after trial of Apostoli's method of electrical treatment, he wrote:—‘So strongly do I now feel on this subject that I would consider myself guilty of a criminal act were I to advise my patient to run the risk of her life—and such a risk—before having given a fair trial to this treatment, even though I were sure that the mortality would not be greater than that which hysterectomy has given me in my private cases—under 4 per cent.’

This is a declaration which must be regarded as phenomenal, coming as it does from a man who is known to observe scrupulously, to think calmly, to reason logically, to decide deliberately, and to act conscientiously. It ought to be, one would imagine, sufficient to check the folly of reckless, indiscriminating laparotomists, and to make their imitators hesitate before risking human life; and it gives matter for grave reflection to all those who have to bear the responsibility of advising and cautioning in such cases.

Dr. Keith tells me that he has not since done more than three hysterectomies (for fibrocystic tumours only) and one castration. This persistent abstention implies his own emphatic condemnation of his former practice, which, as he says, always ‘vexed him with anxious doubts and fears,’ and at the same time it indicates hopeful confidence in the alternative method of treatment he has adopted. Under the circumstances an attitude of watchful expectancy is what is most fitting; while everyone who has gone through the trial of dealing with the perplexities of giving counsel and acting in these cases, must hope that further evidence will establish the fact that in electricity we shall find a resource which, if it does not supersede the knife, will render the necessity of its use much less frequent.

A careful and unprejudiced examination of the published record of the cases which Dr. Keith has treated electrically will show that his resolution to withhold his hand is fully justified by the results of his new practice.

Of the 106 cases reported in detail in his book, published August 1889, eighty-five, or four-fifths, were either restored to health, and in some instances enthusiastically grateful for having escaped an operation, or so much relieved of most of their urgent symptoms that, though content with the improvement already experienced, they were anticipating further progress, under a continuance or renewal of the sittings.

Among the remaining twenty-one, three died during or after the treatment from other diseases; very slightly, if at all, more than the present proportion of deaths in the population at large. One patient who had long suffered from exhausting hæmorrhage, when so much relieved that she could go about, died from a fresh attack, brought on by imprudence, and imperfectly treated during Dr. Keith's absence. Some eight or nine of the seventeen other cases were improving after a small number of applications, but discontinued their visits for various private reasons. Two or three had not the patience to persevere, and in four or five instances no satisfactory benefit could be obtained. Only in one case has the tumour entirely disappeared. Generally the patients had to resign themselves to carrying a diminished and less irksome burden, and this, with the freedom from pressure-symptoms and restored health, they made light of.

It is too soon to speak of the durability of these ameliorations. On this point we must turn for information to the practice of Apostoli. An examination of such of his early cases as she was able to find out in a limited time was made this summer by Dr. Felicia Jakubowska. In her thesis, '*Des résultats immédiats et éloignés du traitement électrique des fibrômes utérins*,' she states that of thirteen patients whom she discovered, after from four to seven years after the completion of their treatment, ten were in the full enjoyment of the relief given by the electricity. Three complained of some insignificant symptoms, but were in a much better state of health and had more capacity for work than before they were treated. These cases were taken indiscriminately as they could be found after so long a

lapse of time ; and if they may be looked upon as a fair representation of the 531 reported at Berlin last July, they testify convincingly on the point of permanence.

The general conclusions we may draw from the observations of those who have made the electrical treatment a subject of intelligent study, are :—

1st. That the almost invariable result of the electrical treatment of fibroma or myoma of the uterus is a marked restoration of the general health.

2nd. That in the great majority of cases it arrests hæmorrhage within a short space of time ; that in certain other cases the cessation of bleeding is produced more slowly.

3rd. That the pain is generally relieved, though not so certainly as the hæmorrhage is stopped.

4th. That the tumours mostly undergo some diminution of bulk ; that in rare cases they disappear ; that when they remain their mobility is greater and they cause less inconvenience ; that failure to arrest development is exceptional ; that in cystic fibroma it is comparatively useless.

5th. That, as a rule, the retrogressive changes produced remain permanent, and that the health continues good.

6th. That the treatment does not render the patient less fit for subsequent myomectomy if circumstances make it necessary ; on the contrary, it rather facilitates the operation, by lessening bulk and loosening adhesions.

Testimony from several quarters seems to have established the fact that the electrical current, when properly applied as directed by Apostoli, has been of utility in large numbers of cases of myoma of the uterus. But its employment is neither easy nor safe in untrained hands. Great diagnostic discrimination is also required before putting the patient under the treatment.

We gather, however, from the best authorities that we may safely recommend it in cases of hypertrophied uterus, of the uterus impacted in the pelvis by peri-uterine deposits, and of interstitial and broad-based tumours.

The process must be expected to be more tedious and uncertain where the tumour is hard and subperitoneal, and there is less probability of good being done when strictly pedunculated ; though in some of these cases a change for the better has been produced.

Little effect can be obtained on the fibro-cystic tumour; and concurrent disease of the uterine appendages is a hindrance to the use of the current. With very large, hard, subperitoneal tumours we can give little hope of more than a partial reduction of size, so as to make them tolerable. But in most cases, and especially in those attended with hæmorrhage, we may advise the use of electricity with the assurance of a cessation of the bleeding, and such a recovery of health as Apostoli calls 'a symptomatic cure.'

There must, however, remain cases where no surgical treatment is required, others where the effect of removing both ovaries has to be carefully considered, and some others where social and other considerations lead to the advisability of early removal of the tumour.

BATTEY'S OPERATION OR OÖPHORECTOMY

In some large uterine tumours, where electricity is not likely to succeed or has failed, menstruation may be stopped by removing the ovaries, and involution of the morbid growth may be reasonably hoped for.

Removal of the ovaries to prematurely stop menstruation was a philosophical suggestion brought forward by Blundell as long ago as 1823. Carried out as it was by Battey, after 1874, restrictedly and with all due precautions, no objection could be made to it. Hegar's adaptation of the operation to cases of uterine fibroids was just, but of limited application. The proposal to extend its use among patients having no tangible disease, but with symptoms difficult to manage, was dangerous. I was far from disposed to put any obstacle in the way of any legitimate operation. I knew, by my ovariectomy experience, how harassing it was to persist, even when right, in the face of prejudiced opposition, and what acute pain may be caused by unfair imputations. But here there was so evidently a perilous temptation in the way of loose professional morality, that, in 1884, I felt bound to express myself thus: 'Though I accept the principle, I am sure that the operation has a very limited application, and is so open to abuse, that its introduction in mental and neurotic cases is only to be thought of after long trials of other tentative measures and the deliberate sanction of

experienced practitioners. Mortal diseases admit of mutilating and desperate remedies. But mutilation for the sake of terminable maladies (which are the fruits of a vicious civilisation or a reckless procreation) is rather a question for the moralist than the surgeon.' It was not without reason that I did this. At the date of the London meeting of the International Medical Congress, Battey, who in the course of several years, and with almost unbounded opportunity of selection, furnished him by a sort of consultative epidemic which followed the exposition of his principles, had only found fifteen cases in which he could see occasion for practising his operation, expressed surprise at the supposed necessity for its frequency in our country. My own experience, and that of many men of repute, forced me to coincide with Battey's reserve and moderation. But an acquaintance with the sectional medical literature, and especially the journalistic publications of the last ten years, leaves the conviction that the contrary propensity is alarming the profession. The constant reproduction of vaunting lists of unsequelled oöphorectomies is an accumulating proof of a mischievous activity, unless it can be explained away by the supposition that some of the authors are deluding themselves into the belief that they have actually done all that they were writing about. The marked attempt to specialise and isolate gynæcological proceedings is an unfavourable manifestation, and creates a fear that in the absence of restraint the profession at large will have to bear up against the rebound of adverse public feeling. When in society and at the clubs it is impossible to avoid sarcastic allusions to the eclipse of the common sense of the consulting-room—when it has become not uncommon to witness in women, far below the stage of matronhood, a flippant familiarity with the jargon of gynæcologists, and a proclivity to yield to some of their vagaries—when at the domestic hearth it is painful to hear reflections upon a suspected relaxation of professional moral integrity—it is beyond a doubt that some pestiferous influence has been at work which justifies the sneers and misgivings of men, and accounts for the newly manifested perversity in women. Some men appear to forget that the mental soundness and purity of our patients is presumed to be as much in our keeping as their bodily health, and that our honour binds us to respect the one as much as to tend the other. We must

admit, it is true, that the number of such delinquents is as yet very small—but a speck of the evil leaven is capable of entraining followers in many questionable courses and inflicting much moral evil upon society. It is certain that there have been indiscretions; that the operation of oöphorectomy has been injudiciously performed without due explanation of its consequences, and with mistaken prophecies of insanity or early death; that in certain circumstances the bounds generally recognised as those which should limit deliberate and consultative practice have been overstepped; that many young women who have been saved from unnecessary mutilation have afterwards borne children; that recoveries from the operation have been incorrectly counted as cures of the diseased condition, and that all failures have not been recorded. All this has excited a suspicion in the public mind—and more than a suspicion in the mind of the profession—that some of the recent expansions of abdominal surgery have not increased public respect for the profession, and require denunciation. I abstain rigidly from any personalities, and take my stand on general evidence, and the unquestionable existence of an uneasy feeling widely abroad. My early warning was partially unheeded. I now make a sturdy and urgent protest against any abandonment of the true principles of professional conduct, and against the abuse of an operation good in itself and valuable when wisely adopted, and I am sure that the sober-minded and right-hearted majority of our profession will emphasise that protest in no measured terms.

Let me repeat that in properly selected cases of innocent uterine tumours, removal of the ovaries is undoubtedly useful, and has been done in a few cases by me with satisfactory results, and much more frequently by other surgeons. But there are other cases where social and other considerations lead to the advisability of early myomectomy or hysterectomy.

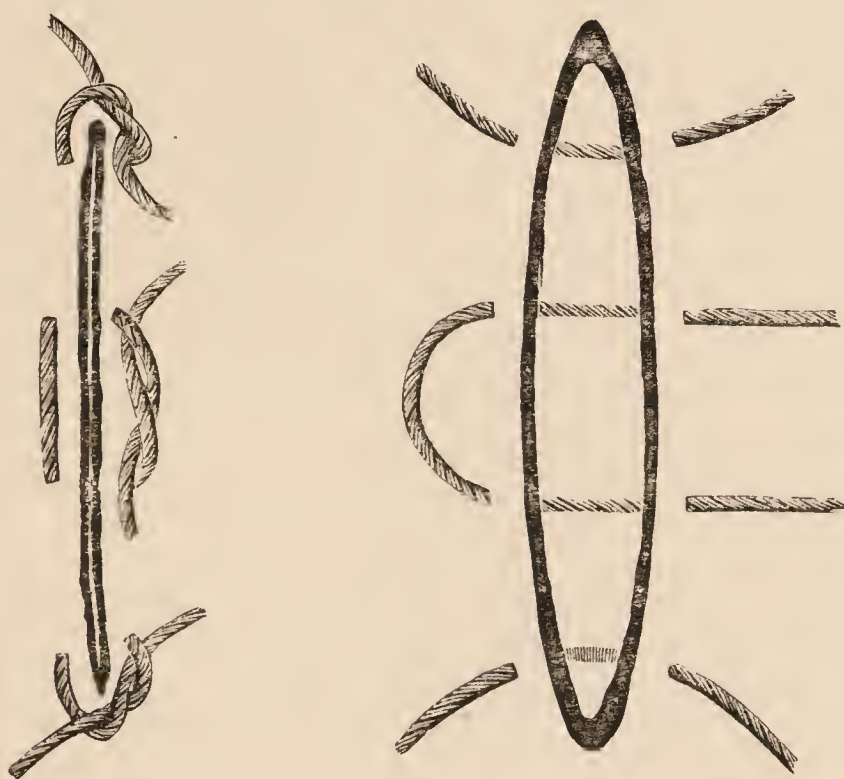
One of the specimens on the table is of historical interest as the first in which a uterine fibroma had been recognised before operation and successfully removed by abdominal section in any British hospital. I operated in June 1871. The patient was then forty-six years of age. She had enjoyed good health until a few months before, and died nineteen years after the operation, of some cardiac and pulmonary disease, without any return of

abdominal trouble. The tumour was surrounded by fifty-nine pints of peritoneal fluid, which had produced a large vaginal rectocele. The tumour was separated from the right side and back part of the fundus uteri with an *écraseur*, but it was afterwards necessary to stop bleeding by a large pin and twisted suture, fixing the stump like a clamp outside the abdominal wall. The patient lived until this year, and died of some chest disease.

Such cases are undoubtedly much less frequent than they were only a few years ago, but still they do occur, and the mode of dealing with the pedicle, or the seat of connection with the uterus, becomes the most important of the operative questions. In England the extraperitoneal treatment by pin and *serre-nœud*, by elastic ligature, or the clamp, has so far yielded better results than intraperitoneal ligature. In Germany the reverse is the case, and I cannot help thinking that, as in ovariectomy the clamp at one time led to better results than the ligature, but gave way to intraperitoneal methods, so it will be with myomectomy. But this is a matter for further observation; and improvements in the mode of applying the ligatures will no doubt be suggested.

The principle which I from the first insisted on, of uniting not only *edges* but flat *surfaces* of peritoneum when closing the opening in the abdominal wall in ovariectomy, became of even greater importance in closing the uterine wound in Cæsarean section and the divided edges of the peritoneal coat of the uterus in myomectomy. In my comments on the case where I first closed the uterine opening in Cæsarean section I contended 'that the peritoneal edges of the divided uterine wall should be carefully brought together, like the parietal peritoneum of the abdominal wall, by many sutures, or by uninterrupted suture along the whole extent of the gap.' Säger carried out the principle more completely by the use of a double row of sutures, and in his earlier operations by the removal of any sub-peritoneal uterine tissue which interfered with the complete apposition of the two peritoneal *surfaces* formed by the inverting of the two peritoneal edges. In his later operations he has been content with bringing *the edges* into accurate contact, and I have recently done this very successfully in myomectomy, only using one row of sutures when this was sufficient to stop bleeding and bring

the peritoneal *edges* accurately together, but preferring a second row when the edges could be turned inwards and the two serous surfaces fastened together by superficial suture—whether interrupted or uninterrupted mattering very little. The two rough sketches show what I have done very successfully in two recent cases of myomectomy. Last June, assisted by Mr. Doran and Dr. Westland, I removed a very dense fibro-myoma, weighing nine pounds nine ounces, from a lady aged thirty-three. It was attached on the right to the fundus uteri by a pedicle about two inches in length and breadth and one in thickness. This was easily secured and tied by triple ligature as in ovariectomy, but after removing a smaller sessile outgrowth not larger than an apple from the left of the fundus, where the uterine tissue was



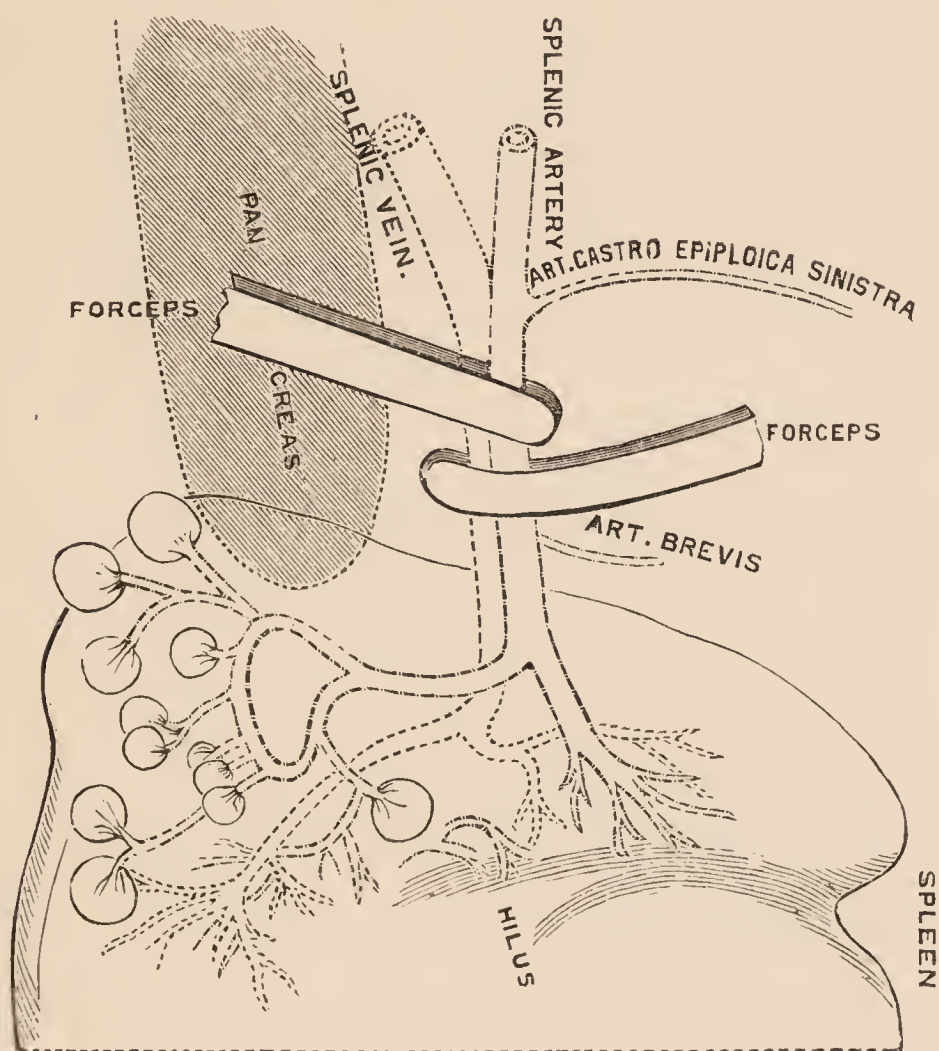
very friable, the ligatures cut through it, and there was very free bleeding. Each end of the divided uterine wall was closed by a single suture, and then other sutures were passed, as shown in the cut, which on being tied pressed the bleeding surfaces firmly together. I carried out the same plan last October with Mr. R. Priestley in the case of a lady from New Zealand. A fibro-myoma projected upwards and backwards from the fundus uteri. It was not easy to define exactly the borderline between myoma and fundus, but I transfixed with a long pin, tied an elastic ligature between the pin and the fundus, and cut the tumour away. I had thought of extra-peritoneal treatment of

the stump, but there was so much traction on the pin that I removed it and the rubber cord. There was very free bleeding, which I could not stop by ligature or torsion, but did so without difficulty in the manner just described. Both patients recovered without anything worthy of record, much more quickly than they could have done under any mode of extraperitoneal treatment.

CÆSAREAN SECTION AND PORRO'S OPERATION

The mortality of the Cæsarean section since the use of the uterine suture and antiseptics has been so much reduced, especially by Säger and Leopold, that previous comparisons with Porro's 'utero-ovarian amputation completing Cæsarean section' (as Porro himself defined his operation) must be corrected. Before 1865 Cæsarean section was a very fatal operation; few children were saved and fewer mothers survived. Porro's amputation certainly lowered the mortality, but after 152 cases had been carefully recorded the mortality was 56 per cent. of the mothers. Of the children 96 were living, and it was said with satisfaction that 162 lives had been saved by 152 operations. On the table before you are the uterus, both ovaries, and a uterine fibroma, which weighed nine pounds, which I removed after delivering a living child at full term in May, 1887, from a lady, aged 37. She and the child are still quite well. Mr. Scattergood, of Leeds, wrote to me last week saying: 'She does not suffer the slightest inconvenience as the result of the operation, indeed feels much better now than she did some years ago. Her boy is also very well.' This case, and the mortality of the Cæsarean section up to that time, led me to a conclusion favourable to Porro; but more recent statistics of the Cæsarean section in Germany teach us to prefer the more conservative operation in cases where there is no uterine tumour. If there be any considerable tumour it would almost always be preferable to remove it with the uterus, but in simpler cases to be content with Cæsarean section as perfected by Säger and Leopold, which has recently given almost as good results as average ovariectomy—the latest returns of the work of a few experienced operators giving a mortality of only 5 per cent. Surely this is a result of which modern Abdominal Surgery may be justly proud.

In order of time, the spleen was the first of the abdominal viscera which was dealt with like an ovarian tumour. But I have little to add to what I have before published on SPLENECTOMY except that the operation is more often done, and that the fears raised by recent physiological views as to the function of the large splenic cells found to contain many half-digested red-blood corpuscles, have not been justified in the two successful cases which I have been able to record. Both patients remain in perfect health, and there is nothing in their blood to show that



they have been without a spleen five and three years respectively. The only practical remark I have to offer is, that in a very large proportion of published cases primary hæmorrhage has been the cause of death, and I think I have provided a safeguard against this danger by the use of pressure forceps in the way shown in the diagram. The blades of the forceps may be protected by a thin sheath of india-rubber. It is possible that an elastic ligature might serve the same purpose as forceps, but it would not be so easy to apply, and there might be danger of slipping.

LIVER AND GALL BLADDER

With regard to the liver and gall bladder, I can only say now that, although many successful cases are on record of removal of gall stones and subsequent closure of the gall bladder by sutures, as well as of fixing it to the abdominal wall and forming a fistulous opening, the impression I had formed and made known five years ago, that excision of the gall bladder—or cholecystectomy—is a better practice, has been strengthened; and I can almost foresee that the general rule in future will be to expose the gall bladder, empty it by a syringe, raise the liver, protect the stomach and intestines by sponges, tie the cystic duct with two ligatures, divide it between them, separate the gall bladder from the liver by knife or scissors, and then close the abdominal wound.

OPERATIVE SURGERY OF THE KIDNEY

Perhaps no other department of Abdominal Surgery has been so rapidly developed during the last few years as that of the kidney.

Nephrolithotomy and nephrectomy may now be regarded as remarkably successful operations. I have nothing to add to what I have before published, but I may refer to a most gratifying case about to be discussed at the Royal Medical and Chirurgical Society, where Mr. Clement Lucas, in July 1885, removed the right kidney from a woman thirty-seven years of age on account of long-standing hæmaturia. For some three months she was free from pain and bleeding; then a sudden attack of pain in the left loin was followed by complete suppression of urine, and death from uræmic coma was imminent, when Mr. Lucas cut down on the remaining kidney and removed a stone which acted as a ball-valve at the top of the ureter. The woman was rescued from imminent death, and has for the last five years enjoyed good health. As the lumbar incision was adopted in this case, some might hardly consider it abdominal surgery. But so large a proportion of nephrectomies are done by abdominal incisions, that I may be excused for mentioning a case of which the operator might well be proud, and congratulate him upon his

delay in publication until the patient's recovery has been well established.

ENTERECTOMY

What would Astley Cooper or Abernethy, Lawrence, Syme, or Brodie have said if a proposal had been made to cut away several inches of intestine, and, after uniting the divided edges of the upper and lower openings together, to return the intestines, and close the opening made in the abdominal wall? They all regarded a mere wound of intestine as almost certainly fatal, and thought life could only be saved by forming an artificial anus. How amazed any of these great surgeons, or any of their contemporaries, would be if they could see the specimen I now show you, which has been kindly lent me from the museum of St. Thomas's Hospital! A boy, aged fourteen, was kicked by a horse one evening, and taken to St. Thomas's late at night. Next morning Mr. Croft, suspecting ruptured intestine and acute peritonitis, at once made an exploratory incision. He found fæcal matter in the peritoneal cavity, and a part of the ileum bruised and ruptured. The piece you now see was separated from its mesenteric attachment and cut away. Makins's forceps, which are here, proved very useful in keeping the canal closed before the intestine was divided, and while the sutures were being passed. The mesenteric wound was closed by sutures, the cut ends of the intestine carefully adjusted, and fastened together by Lembert's sutures. About forty sutures were left in. The peritoneal cavity was carefully cleansed by warm boracic solution. No drain was used. The external wound was closed by silk sutures. Recovery was uninterrupted, and the boy was shown at the Clinical Society nearly a year afterwards in good health. The piece of intestine seen here inverted measures nearly three inches at the free border, but only about half an inch at the mesenteric border. The rupture is only about half an inch in diameter, but it had permitted free escape of fæcal matter into the peritoneal cavity; and if ever a life has been saved by a surgeon, this boy's was by Mr. Croft, and so will be many more if the practice he adopted is imitated.

Lembert's suture was employed in this and in other successful cases; but I cannot help thinking that Mr. Stanmore Bishop's is better. He has described and illustrated it so fully in his

paper published in the *Medico-Chirurgical Transactions*, that I need not now do more than point to the interesting preparations before you to show what accurate and complete union follows its application. The fine needles and silk Mr. Bishop uses are here, and his needle-holder. I must, however, add a word of advice to those who may think of using them—that a good deal of practice in the study is necessary to enable any one to use them with precision and at all rapidly.

My distinguished American friend Dr. Senn, Professor of Surgery at Chicago, has made some valuable additions to the practice of enterectomy by teaching the use of absorbable plates of decalcified bone in facilitating the approximation and union of the two openings to restore the continuity of the intestinal canal. I have some of his plates here, preserved in an antiseptic solution as they are kept for use. They come away with the faeces about a week after the operation. They assist very much in securing complete apposition of the two serous surfaces, and Dr. Senn's method of slightly scarifying the two surfaces with the point of a needle doubtless adds to the rapidity and firmness of their adhesion. Similar scarification might be of use in the uterine suture, and I shall certainly give it a trial.

I can strongly advise all operating surgeons to study carefully Dr. Senn's important memoirs on 'Resection of the Cæcum for Carcinoma,' and on 'The Diagnosis and Operative Treatment of Gunshot Wounds of the Stomach and Intestines.' They abound in original suggestions and in records of experimental and operative work. But I cannot do more now than allude to two cases of intraperitoneal rupture of the bladder which are brilliant examples of modern Abdominal Surgery.

Intraperitoneal rupture of the bladder has been almost always fatal, but Sir William MacCormac had two cases four years ago where he exposed the bladder by abdominal section, carefully closed the rent by silk sutures, washed out the peritoneal cavity by warm boric solution, closed the external wound, and perfect recovery was the result. These are the first of such cases on record. Careful diagnosis and early skilful operation earned their just reward. Silk sutures were passed about a quarter of an inch apart through the serous and muscular coats only, twice on each side of the opening, so that when they were tied the mucous membrane was inverted, and two serous surfaces were

fastened together on the principle I have spoken of before. The peritoneal cavity was thoroughly cleansed by irrigation, a glass drainage-tube introduced, and a catheter fixed in the urethra. These were removed after three days, and in three weeks the man was as well as before the accident.

In a second and similar case the same course was adopted, with the exception of drainage, and recovery was even more rapid than in the first case. Accurate closure of the wound in the bladder by sero-muscular sutures and thorough cleansing of the peritoneal cavity were doubtless the chief points in these two operations. As Sir William MacCormac urges in his very able report of these cases,¹ in cases of intraperitoneal ruptured bladder there is need for 'earlier interference and bolder practice. The whole history of Abdominal Surgery points in this direction.'

Here I am compelled by the hour to close this very hasty and imperfect sketch of what we, the surgeons of the present, have done and are doing, and how we do it. And for our younger Fellows and Members—for the surgeons of the future—may we not be confident that, with the energetic spirit of inquiry now awakened, with an enlightened determination to apply all the resources of modern scientific discovery to the perfecting of our art, with a conscientious aim at making it as truly conservative as is compatible with usefulness and progress, and with honourable feeling and highly cultivated judgment directing hands delicately and expressly trained, we may augur for the surgeons of the coming time an influence supremely beneficent for mankind, and promise to its devotees the dignity and distinction justly earned by their life-giving and health-preserving work?

¹ *Lancet*, December 11, 1886.

APPENDIX

AT the conclusion of the lecture a number of specimens from M. Pasteur, and some prepared by Dr. Ruffer and Mr. Pringle, were exhibited by Dr. Woodhead, highly magnified, and thrown by an oxy-hydrogen lantern upon a large screen. After showing the mycoderms of vinegar and wine, the following were the principal specimens described by Dr. Woodhead :—

- | | | |
|----------|---|--|
| | { | Fungi and bacteria cultivations. |
| | { | The bacillus of pneumonia (Friedländer). Macro- and microscopic appearances. |
| × 20,000 | { | Tubercle bacilli in the walls of a blood vessel. |
| | { | Blood containing (1) spirilla of relapsing fever. |
| | { | „ (2) organisms of fowl cholera. |
| | { | „ (3) „ of septicæmia. |
| | { | „ (4) anthrax bacilli. |
| | { | Cultures of anthrax bacilli. Leptothrix forms. |
| × 30 000 | { | Photo-micrographs prepared by Mr. Andrew Pringle : |
| | { | Tetanus bacilli; long threads, drumstick form, spores, &c. |
| | { | Diphtheria bacilli, two forms. |
| | { | Tubercle bacilli, with and without spores. |
| | { | Anthrax bacilli in mesenteric vessels of mouse. |
| × 25,000 | { | Drawings by Dr. Beadles, made from a number of Dr. M. A. Ruffer's specimens, of microphages and macrophages from adenoid tissue of the intestine and from tubercular spleens. |
| | { | In the microphages, bacilli, red blood corpuscles and pigment could be seen in various stages of degeneration, whilst these microphages, also in various stages of degeneration, were in turn seen contained within the protoplasm of the macrophages. |

CASTRATION OF WOMEN

The following article, which I contributed to the 'American Journal of the Medical Sciences,' appeared in the new series, vol. xcii. It was published in 1886 with articles on the same subject by Hegar and Battey. It is now reprinted in consequence of continued and apparently extending abuse of the unnecessary mutilation of young women :—

CASTRATION IN MENTAL AND NERVOUS DISEASES

Castration, in the wide sense of the word, both of the male and female, has an import which attaches to no other surgical operation. It not only puts in jeopardy the life of the individual on whom it is performed, but it involves the certainty of the non-production of the whole series of beings that might result from man's obedience to the first command of his Creator, 'Be fruitful and multiply.' Its potential fatality, as regards the subject of it, sinks into insignificance when compared with the absolute extinguishment of one line of the species. Hence its gravity among moralists, and the severity with which it has been visited by legislators. Death and penal servitude for life, without remission, are the punishments set by some codes upon the crime of unjustifiable castration ; which term is made to include all mutilations that may put an end to the virility or fecundity of the victim.

The duties of a surgeon often lead him near the confines of what is illegal. With the deceptive plausibilities of patients, their indefinite notions of morality, and his own propensity to action, induced by sympathy with distress, the balance of prudence is sometimes apt to waver in uncertain hands. One is thus brought to see how indispensable strict ethical training is as the complement of technical education, how needed is a check upon the impulses of acquired or reputed manual dexterity, how, in reference to laparotomy operations in general, the profession should be made to feel that it is acting under the restrictive influence of the opinions and decisions of its wisest and most vigilant leaders, and how urgent it is that those leaders should rise to the level of their dignities and responsibilities.

The advances made in abdominal surgery within the last five-and-forty years are, to those who have passed a long life in practice, who have been sharers in the work done, and who have found time to look about them and note what their contemporaries have undertaken, something astounding. Men of to-day, launched upon the full flood, have little idea of how the rush of accumulated experience which

carries them along has been made up ; or of the struggles and perplexities those who were the first to move had in paddling and steering through the swamps of difficulties, and in face of the blasts of prejudice. Progress was slow and there was time for reflection. It is not to be wondered at that such reflection sometimes caused hesitation and yielding before obstacles. Cooper, Lawrence, Green, Brodie were great surgeons. They did and taught surgery that was the boast and honour of their day. But they were orthodox men ; they revered authority, and were authorities themselves. A pause for inquiry as to what was going on, and where it would lead to, was, at their suggestion, not dishonourable and did good service. It helped to enlighten and liberalize them, and it moderated the contagious impetuosity of the new adventurers. It would not be unwise if we made a halt now, and took account of our position in regard to some points that are pressing and open to question. We should be among the last to stand in the way of clear-sighted attempts to move onward, but wish always to be guided, and to see others guide themselves, with caution and by the light of reason.

This is not the place to go into the history of ovariectomy. But it may be well to recall some points in it connected with the subject before us. In its early days the operation was looked upon as a personal enterprise, to be taken up every now and then by men of the Livingstone, Brunel, or Columbus type, who were either vaguely enthusiastic, stimulated by an impulse for out-of-the-way performances, or so wise and so far ahead of their times that few could understand them ; and fewer still were inclined to follow an example which, though it might meet with a certain amount of success, excited astonishment and suspicion more than admiration, and brought little other reward than the consciousness of having made honest efforts to rescue suffering women from impending death. Then came a time when things were different. The profession took up the matter seriously and practically. It was still an assault upon unsolved problems. But the contention ended in the opening up and annexation of the 'whole domain of peritoneal surgery.' It was like the discovery of the Californian diggings or the African diamond fields. The way was cleared for all prospectors, and the benefits spread world-wide. Between the years 1840 and 1865 the excision of ovarian tumours came to be accepted as a sound piece of surgery, as admissible among the arts '*quæ prosunt omnibus*' as lithotomy, and more promising in its results than most other capital operations. It became naturalized in England, was taught in the schools, and soon threw out an abounding crop of controversial and didactic literature.

But there was a reverse to this bright side of things. Inexperience, rashness, maladroitness threatened danger. I saw then for ovariectomy, as we now see for laparotomy, a disposition to wild, irreflective meddling. In my book, published in 1865, I seem to have anticipated something similar to the present folly, though not nearly to the extent it has now gone. Here is what I say in the preface : 'I cannot send forth this volume without a word of caution. A discovery which has triumphed over opposition of all kinds, honest and scientific, prejudiced and ignorant, may still be ruined by the support of rash, inconsistent, thoughtless partisans, whose failures do not reflect so much discredit on themselves as on the operation they have badly performed in unsuitable cases. Indications are not wanting that ovariectomy has entered on this phase of progress, and there is reason to fear that judicious men may be influenced by the outcry of the foolish, and that a triumph of British surgery which has been won by such great labour and care may be arrested before it is complete—may even be converted into temporary defeat—by the indiscriminate support of zealous but injudicious advocates.' We are not wrong in assuming that such warnings—for mine did not stand alone—were not useless. More discrimination was shown in the selection of cases, diagnosis was more scrutinizing, operators fitted themselves better for the work by reading and observation, and both unsuccessful operations and incompleted attempts became less frequent.

But ovariectomy was not only viable and strong. It had in it an unsurmised power of fecundity which we can now estimate by the many prefixes to its terminal dissyllable. Before the present reign, the art of midwifery was somewhat in the shade. The needs of royal maternity gave it the prestige which was wanting to its utility. Knightly spurs and hereditary rank were won in the palace chambers. The title was a little incongruous with the old familiar term 'midwife.' The synonymous 'accoucheur' came into vogue, but it did not accommodate itself to the linguistic requirements of professors and writers, and was objectionable as being imperfectly euphemistic and too directly artistic. Something with a more scientific twang was the desideratum, and 'obstetrician' seemed for a while to be all that could be hoped for by those who were ambitious of showing that they could propound doctrines as well as handle forceps. Yet women, whether in the hands of Shandean Slops or Caxtonian Squills, are not always as they should be, either before or after the great obstetrical event. They have maladies before, meet with accidents at the time, and often suffer consequences which require surgical skill for their cure. Some men could split a perineum, but it was not everyone who could put it right again, and obstet-

ricians soon began to foster a competition for secondary specialties, to indulge in the creation of Greek compounds, and were not long in fitting themselves with the distinguishing appellative of gynæcologists. Gynæcological societies were the inevitable complement of this sectarianism, and in their proceedings we find all their speculations and manipulations so put in evidence that we can leisurely watch and criticise them in their budding, blooming, and fruition.

The growth of specialization in medical science is at the same time a benefit and a peril. It is well to know that men of broad culture, capable of linking each small and special area of research to, and viewing it in the light of the vaster realm in which it is an essential and inalienable factor, devote themselves to particular investigations. And if special gatherings were schools of instruction by masters, instead of theatres, with rapidly recurring exhibitions of curiosities and recitals of marvels, which must be made forthcoming by some means at the appointed times, we could appreciate them. There is a wide difference between one man acting and ruling as a specialist, and a miscellaneous lot of men each pushing to the front and grouping themselves together as a society of specialists. The master of a pack of hounds must be a specialist in his way, but it would be absurd to suppose that every rider in the field would be able to take his place, or any other than that of a follower. But herein is the danger with groups of gynæcologists. It would not answer for all to run on the same track. To be anything, each must hunt up his own little therapeutical quarry and keep to it. Groping among details is an absorbing and paralyzing occupation, and soon the curve of a pessary or the lining of a speculum fills the field of vision, and great principles are lost sight of. With one such idea kept steadily running in the same groove, a man may quickly find his way down to the lowest level of routine womb-scaffolding or singeing. And so it is that while out of the multitude of gynæcologists a few inspired with Hunter's idea, 'all discord, harmony not understood,' are spending their lives in the higher regions of speculative inquiry, thinking, developing ideas, multiplying original principles, and applying them to the pathological phenomena peculiar to the female sex—with a special view to the elucidation of their causes, mode of origin, and prevention—the rest are dispersing themselves over the lower ground of therapeutical action. Disease exists, the how and the why concern them not. Why search into the inscrutable? If the faults and follies of mankind engender excisable matter, their business is with the palpable, and to get rid of it. So myomotomy follows ovariectomy, Porro supplants the Cesarean, Battey breaks through his difficulties with 'normal oöphorectomy,' and the *Moutons de Panurge* are soon

seen flocking over his gap. It is with this gregarious castration that we have to deal.

It is about fourteen years since the operation of normal ovariectomy, as Battey called it, was brought practically before the profession. It is now impossible to ascertain how often, or by how many surgeons, it has been done. But the most recent bibliography of the subject extends to about five octavo pages, and comprises the names of more than one hundred and fifty writers. The greater part of these publications consists of the accounts of cases and the discussion of points of practice. Some of the matter is critical, much of it defensive and exculpatory. So that it has both a history and a literature.

The operation itself is in no sense a novelty. It has been practised in all times, though not for surgical reasons. In the last century, about the same time, L'Aumonier and Pott did it remedially; the first, without premeditation in the course of opening an abscess in the iliac region. Pott, however, intentionally took away two ovaries which formed inguinal hernias. Though Blundell never did the operation, it would be unjust to omit all reference to his so often cited prophetic suggestion, made before the Medico-Chirurgical Society in 1823. Lassus, in 1858, and two German writers, mention some other cases similar to that of Pott. In 1869, Koeberlé, while putting a ligature on the broad ligament for the relief of a retroverted uterus, took away an ovary which embarrassed his proceedings. Esmarch, too, a little while later, removed both ovaries from a young woman who had congenital atrophy of the vagina. The monthly sufferings were so great that each time her contortions forced the ovaries through the inguinal openings. They were the point of departure of all the neuralgic radiations, and Esmarch cut them away as a dentist would draw a troublesome tooth, without theorising about the suppression of function and anticipated climacteric. He simply did a perfect castration and left the titular honours of normal ovariectomy, or oöphorectomy, as it was soon after called, to Battey and to Hegar, who began to use the phrase 'castration of women.'

These two surgeons, in July and August 1872, within twenty days of each other, did the operation of castration by the abdominal section. One was at Fribourg, the other at Rome, in Georgia. Of course, they knew nothing of each other's reasonings and actions. Both their patients were in much the same condition, with menstrual neuralgia. Hegar's patient died of septicæmia, and he held his hand for four long years. Battey had a better chance. His patient got well and was quit of her troubles. Elated by the success of his operation, he hastened to make known what he had done by a notice

in the 'Atlanta Medical and Surgical Journal' the September following, and six months afterwards gave all the details of his performance, and defended his theory, before the Medical Society of Georgia. Both men had the same idea, or nearly so. Esmarch's object was nothing more than the removal of the organs that were the seat or the cause of pain in his patient. It was a question of function and constitutional effects with the two pathologists. Hegar, from the first, explicitly stated that what he hoped to do by castration was to bring about a suppression of the ovarian function, a cessation of the periodical and intermittent influence of the ovaries on the whole system, and an early declaration of the menopause. Whether Battey went as far and was as clear in his conception of the import of the proposal has been doubted. He had taken a long time—six years—to deliberate and to consult about it, and met with nothing but indifference and disapproval. His arguments brought no one over to side with him. It was said that his idea was merely that of calming down pelvic disturbance, without calculating upon further consequences ; but he distinctly mentions that he expected a 'change of life' to follow the castration. If he did not argue out the matter, and expound his doctrine, with German elaborateness, we may at least admit that he knew what he was about and had considered what was likely to happen. At any rate, he thought out his subject carefully, acted independently, and was the first known to do so. He was prudent, waited patiently, and watched assiduously for a fair occasion to put his proposal to the test, and at last succeeded in showing it to be not only logical and rational, but effectual in practice. If his arguments had made few converts to his opinions, his practice was soon adopted by followers, and he is entitled to the credit of originality.

Up to this time we had heard only of menstrual difficulties as a motive for oöphorectomy. The ovaries were treated as confirmed and convicted culprits. Battey seems to have spent as much as six years in reformatory efforts with his first case. It was only when all looked utterly hopeless and incorrigible that the extreme penalty was resolved on and carried out. It was a sorry alternative, and not one to boast of. When a surgeon is obliged not only to put on the black cap but to become the executioner, the only redeeming point in the business is the skill he may display in carrying out the sentence. The blot is the necessity for such a measure. As society is wanting in reference to crime, so the profession is wanting in reference to disease. There is too much law, and not enough gospel ; too much doctoring, and not enough philosophic pathology. It might be otherwise. With better principles and training we should see less of crime and its consequences. With a

keener estimate of the higher functions of medicine, more thinking, more research and systematic dialectical reasoning, there would be more defiance of disease, more life-giving power, and less of surgery. But we are not yet at this point, and dysmenorrhœal invalids may in the meantime be thankful that there are still some oöphorectomists as considerate and merciful as Battey.

The year 1872 was remarkable in that, within a month of each other, three oöphorectomies were done by three different surgeons, in three different countries, without either of the three being aware of what the others were about. We have noticed the operations of Battey and Hegar. On August 1, a few days after Hegar's operation, Tait, of Birmingham, is reported to have also removed two ovaries from a woman who was sinking from irrepressible hæmorrhages due to uterine enlargement or tumour. She recovered and was better two years afterwards. In the course of the next year it is also recorded that he did three more similar operations. In two of the cases he took away only one ovary. That was imperfect castration—not the complete operation of Hegar. The want of appreciation of Hegar's motive for the operation is evident. Of a large proportion of Battey's early operations, the same defect is also apparent in the tables. It would seem that he too had failed, from some unexplained reason, in fidelity to the principle upon which all rests.

Gillmore and Pallen, Americans, did the bilateral operation in December 1872, with success. In the next four years many other names came upon the lists, as Peaslee, Trenholme, Goodell, Sims, Thomas. These for the most part followed Battey's example, and at the beginning operated on patients with ovarian neuralgia and general nervous symptoms, or with some congenital imperfections interfering with menstruation.

Then, in 1876, Trenholme did as Hegar and Tait had done, and used the operation for hæmorrhage, depending upon uterine fibroids. Later on, operators found all sorts of pretexts for operating. Too many of their operations were imperfect. Of eleven operations done by Sims, between February 1875 and August 1881, six were unilateral. Nor was the mode of operating determined; some choosing to do the removal by the vagina, others by the abdominal section. At length, in 1881, Battey, deploring the abuse of his operation, when at the International Congress held in London, felt himself constrained to renew his protest, and record the fact that up to that date he had met with only fifteen cases in which he could see reason for carrying out the practice.

It was not until 1876 that Hegar, in Germany, recommenced operating. In August of that year he removed the ovaries from

two women who had hæmorrhagic fibroids. Both were saved and benefited. Kaltenbach then, in the following October, operated under the same conditions, but his patient died. Nussbaum had a success the same year. Between that time and 1879, Martin, Freund, Fehling, and Tauffer castrated several women, mostly for fibroids that could not be otherwise treated. The idea in Germany was, that, as Hegar pointed out, this was the most legitimate use of the operation. Tauffer and Langenbuch thought the practice might be extended, and operated in some instances in which the dysmenorrhœa and other symptoms were manifestly of ovarian origin. But upon the whole the influence of Hegar's doctrines prevailed, and no such operative outburst as took place in America was seen in Germany. The reports of Wiedow, at Fribourg and Copenhagen, show how important have been the amount and success of oöphorectomy in uterine cases among the Germans. In Switzerland, Bischoff, Bircher, and Müller took up Battey's notions and practice without hesitation. Since 1880 the operation has been accepted in Spain and Italy.

It is not the first time it has been said that there are some things which the French manage better than we do. And certainly in some gynæcological matters it has been so. The logical faculty is strong in Frenchmen. It would almost seem that their women are not to the same extent as others liable to pelvic troubles. The subject of oöphorectomy had become repulsive from the fanaticism in America, and perplexing from some of the English revelations. Batteyism was on its trial and undergoing a process of classification. There was time to wait. *Fiat experimentum*—they could be content for a time with observations and reports. So things went on till 1880, when, convinced of the rightness of the principle, and that the operation in the hands of judicious men was being turned to useful account, Professor Duplay operated in a case of fibroma, taking away both ovaries. His second operation was in 1884. Péan began in 1882, and has altogether reported eight operations. Three of his cases had congenital absence of uterus and vagina. The five others were neuralgic. Though, in general, abdominal sections are not popular among French surgeons, they are now carefully choosing their cases for oöphorectomy. The usages of French surgical practice render an epidemic of laparotomy very unlikely.

The operation was not at first well received in England. Tait, of Birmingham, has been identified with it from the beginning. He has modified it and extended its application. Many others have followed in his steps ; some have tried to outstrip him. The ovaries and all their appendages now go the same way ; and the meshes of the physical, mental, and moral network of reasons why the operation should be done are so closely woven that few cases of a per-

plexing nature, that can anyhow be connected with the generative organs or functions, have a chance of escaping laparotomy or something more. The present state of oöphorectomy in England proves how fully justified I was in writing as I did in 1882, and shows how little my warning has been attended to. I said then, and I have not a word to retract now : ‘Though I accept the principle, I see that the operation has a very limited application, and is so open to abuse that its introduction in mental and neurotic cases is only to be thought of after long trials of other tentative measures, and the deliberate sanction of experienced practitioners. . . . Except in cases where bleeding fibroids may call for the extirpation of the healthy ovaries, we might at least require some evidence of the ovaries being diseased before consenting to their extirpation in the hope of curing any of those vague nervous disorders to which women are so subject, which are often dispelled by moral treatment or social changes, often benefited by measures which can have but little effect except on the imagination, often return after cure in any way, and leave the hopeless beings the prey of unscrupulous or illogically enthusiastic experimenters.’

The danger is now increasing as the operation is becoming world-wide. The oöphorectomists of civilization touch hands with the aboriginal spayers of New Zealand. The ovary is, in fact, the nucleus of gynæcological science and the source of gynæcological practice. Its products give occupation to the obstetrical art. The disturbances it sets up in the system at large are the prairie grounds of gynæcological proletarians. The morbid structural changes, displacements, and accidents of it and its appendages are the arena of its operators. Wonderful, indeed, is woman’s hydra-like tolerance of sections and mutilations under their hands !

But the ovary is more than this. Reproduction is the dominant function of woman’s life, and all her other living actions are but contributory. Physiologically and pathologically, the generative organs have peculiarities of which the surgeon must take account. They are not vital organs. The purpose they serve is more in relation with the species than the individual. Their life of functional activity is not of the same extent as that of the being of which they form part. The bodily health is none the worse, perhaps better, during the time of their quiescence. Disease is exceptional in them before puberty. The time of their activity is the time when they are most often attacked by disease. The origin of diseases in them which prove fatal after the change of life may almost always be dated back to the middle period. They affect both the physical and mental powers and qualities during the time they are in development and full play ; not much before or after.

The tubes, womb, and vagina are accessories more or less essential. The ovary may exist without them ; but it is seldom that the tubes, womb, and vagina are fully formed in the absence of ovaries. There may be an evolution of ova when the appendages are no longer there or are even cancerous. When the ovaries are diseased or removed function stops, and the other parts shrink up. None of the subordinate parts of the passages can do what the ovary does, but the tubes may supplant the uterus as the seat of gestation, and the uterus, except during pregnancy, acts chiefly as a section of the tube. In infancy and old age its cavity is contracted and approaches nearer the form of the tube. There is a fact, too, which should never be neglected by the surgeon in coming to a decision about the operation of excision, especially in cases where the indications are not positive—to wit, that the ovaries are not such isolated organs, nor so invariable in their number, as is generally supposed. There, as elsewhere, is shown the tendency to revert to lower types of development ; and patches of ova-bearing tissue may exist in adjacent peritoneal folds. These may be beyond the ligature of the pedicle ; and, if left, keep up the sexual impressions and power of periodical ovulation. Liégois mentions this subject in his ‘*Physiologie appliquée à la Médecine et à la Chirurgie*,’ of the date 1869. Waldeyer found a piece of ovarian tissue in the pedicle of a tumour after having, as he thought, finished an ovariectomy. Weigel counted, out of six hundred women examined, no less than twenty-three with more than the ordinary two ovaries. Instances of regular menstruation, and even of pregnancy, after double ovariectomy, have been met with sufficiently often to show how easy it is for the expectations of a surgeon to be thwarted by a condition which he can neither foretell nor determine exactly at the time of his operation.

The fact that women remain for some five-and-thirty or forty years with a certain part of their organization in a state of periodical excitement, ready for the special act of childbearing, would lead *a priori* to the supposition that the whole of that organization must in some way or other show the effects of it. And such is the case. At an early age females are more tenacious of life, and the mortality of boys is greater than that of girls. During the procreative period the excess is on the side of the females, independently of the mishaps of childbirth. After the climacteric, male deaths are in greater proportion. The disproportion in the number of the two sexes in the living population would be still more marked than it is, were it not for the casualties and diseases connected with maternity. There is a constitutional difference between the sexes at all times ; but during the time when women

are essentially females, they have more sensibility and excitability, a more lax and delicate fibre, with a strong tendency to nervous affections and diseases of an asthenic character. The development of puberty produces one set of disorders, usually anæmic ; ovulation, parturition, and lactation give rise to another class ; while nervous anomalies and degenerations of tissues mark the decline of the functions and the torpidity of the organs of generation.

So far as regards the many diseases, the relief of which by castration has been either proved or postulated, we may virtually put children out of the question. They may occasionally require ovariectomy, but with that at present we have nothing to do. In one of the most recently published tables of oöphorectomies, the earliest age among the patients operated on for hæmorrhagic myoma was eighteen, for ovarian neuralgia and hysteria seventeen, and she was none the better for it, and the youngest on the malformation list was eighteen.

Péan once castrated for epileptic symptoms with mental disorder at the age of forty-five with no good result, and this is the most advanced age mentioned in the table of nervous patients. The age next below that was forty. Fifty-two is the extreme age in the table of uterine cases, and the patient died some months afterwards of cancer. It would thus seem that in all the older patients who submit to abdominal section, it is ovariectomy for cystic or other enlargement of the ovary that is done. Out of 171 cases undergoing the operation for hæmorrhagic uterine fibroids, 53 were between thirty and forty years of age, 62 between forty and fifty, and only 9 below thirty. The number of cases of oöphorectomy for other causes is comparatively small, and few of them outside the middle age. The limits of our investigation of the diseases requiring oöphorectomy are thus drawn within the narrow compass of the twenty years of woman's life between the ages of thirty and fifty. The find here cannot, in the common run of things, be very rich except for fibroids. If the apostle of the doctrine could accept only 15 cases as fit for the operation in the space of ten years, we may assume that, in any great advance upon that proportion there must be either some self-deception or some want of judgment.

During the twenty years just named, the most common disease to which women are subject, connected with the organs of generation, is an abnormal development of the uterine muscular or fibroid tissue, either interstitial, or projecting from one or other of the uterine surfaces. Such tumours occasion discomfort often so great as to incapacitate for social life. They are impediments to child-bearing. They give rise to a variety of reflex nervous symptoms, sometimes almost insupportable. They may cause ascites or set up

acute or chronic peritonitis, and dangerous hæmorrhages are a notable consequence. Occasionally their pressure on the large vessels, nerves, lymphatics, ureters, rectum, and bladder has been fatal. And Roehrig declares that, among the cases bad enough to resort to Kreuznach, there has been a direct mortality of 11·4 per cent.

A disease against which stands such a formidable array of accusations calls for all the resources of science to remedy it. Although many women suffer less than can be easily explained, it must be the misery and ruin of a long series of existences. Medical treatment does not count for much. The results of myomotomy are deplorable even now, and the operation, when not fatal, has often been incomplete. The tumours have, as it were, a sort of gregarious habit, and the patient is left liable to a relapse to her former state, either from the growth of those abandoned, or from new formations. Many writers have maintained that one out of every five or six women has uterine fibroids of some kind, large or small, unheeded or troublesome. Fortunately, if it be so, the greater part of them have only a transient existence. The numbers that turn up in a state of degenerescence or transformation in the autopsies of old women show that they have a tendency to shrink up, or become innocuous, if there be a survival of the menopause.

Based on this fact, castration, as compared with myomotomy, presents us with the striking contrast of a mortality of only 14·6 per cent., a diminution of the tumours, a stoppage of the hæmorrhages, and a disappearance of many of the accompanying symptoms. Moreover, as half this mortality has been due to septicæmia, there is here a wide field for surgical enterprise.

But the uterus is only a section of the ovarian outlet, destined for the temporary sojourn of the embryo. We have not exactly determined what are the influences on the constitution of the natural functions, and do not know much of the pathology of the strictly efferent part of the tubes—the oviducts or the Fallopian tubes. Sometimes they usurp the incubatory office. Then, no doubt, as death is otherwise mostly inevitable, the best thing to do is to extirpate them as soon as we can be assured that their condition is menacing life. But latterly they have had as sorry a time of it as the London dogs. They are erectile structures, and blushed at the touch of the abdominal explorer. A cry was raised against them as subject to congestions, dropsies, constipations, purulencies, hæmorrhages, neuralgias, and as the propagators of all sorts of psychical aberrations. Then followed a savage raid, and every hypogastric malaise incurred an exploration or a sacrifice. As with other tubes, the inlet, passage, and outlet are not always as free as they should be, and the contents are not always normal. Irritated

themselves, they cause irritation elsewhere. As much may be said of and against similar things in the male. But would anyone strip off the penis for a stricture or a gonorrhœa, or castrate a man because he had a hydrocele, or was a moral delinquent? It is better to be candid and patient, say we know but little, strive to learn more, and in the meantime abstain from doing mischief. Who can diagnosticate with certainty the presence of irreparable disease in these out-of-the-way organs? An exploratory incision is an avowal of ignorance, and too often the expedient of impatience. How far is it justifiable as a means of diagnosis in diseases short of fatal, where there is time to wait?

A case of congenital malformation, with what is now sometimes called 'obstructive dysmenorrhœa,' ending fatally, under his care about the year 1866, was the inspiring cause of Battey's reasoning on the subject of the extirpation of the ovaries as a remedial measure. Gillmore was the first to put his conclusions to the test in reference to this particular point. Peaslee followed in 1876, and Battey in 1877. Battey had the satisfaction of a good result. So too it was with Gillmore's case. Since then the operation has been done for the same or similar reasons, so far as we can judge from the small numbers, with a mortality of about 15 per cent. In this category of cases we may place obstructions to the menstrual functions acquired in severe labours, by accidents, by gynæcological attempts at surgery, or occasioned by pelvic distortions, flexions, and displacements of the uterus. They are, after all, not very common, and oöphorectomy, as a means of getting rid of the difficulty, is less dangerous and more certain than the other operations done to relieve or gratify the patients.

The ovaries themselves are often the seat of pain, and the cause of acute neuralgia and hysterical symptoms. The attacks occur with the periodical excitement of the organ. Every now and then such symptoms show their connection with it by being the accompaniment to impregnation, and to some women the proof of conception. They often attend the early stage of growth of cystic disease. Repeatedly, a cessation of the attacks of pain has followed the operation of excision. This was the result in three out of four operations that I have performed. Cause, effect, and remedy are here plainly demonstrated. As to my two other operations, in one it was probable that the benefit was as much owing to the reposition of a thickened and retroflexed uterus as to the taking away of one of the ovaries, the other being in such a state of atrophy that it had no outlines, and was inert. In my fifth case the two ovaries had been amputated by surgeons of renown in Holland, at different times, without permanent benefit. At my operation there was no

trace of another ovary, and what I did was to separate part of the omentum and a coil of small intestine from the uterus, to which they were attached, and to divide another piece of omentum which adhered to both the fundus uteri and the cicatrix in the abdominal wall. Here the two castrations did no good. The liberation of abnormal connections near the seat of pain did what was wanted, and must be regarded as something more than the completion of the two oöphorectomies. All these cases had been discussed at repeated consultations among a number of experts; nothing was overlooked in the way of palliation, and no decision was taken without unbiassed deliberation. Even this small experience shows how this subject of operation for nervous dysmenorrhœa is surrounded with difficulties both of diagnosis and prognosis. In looking at the vast multitude of patients who come under professional notice, with a medley of nervous or mental symptoms so tantalising by their refractoriness and their inexplicability, we can understand how it is that many hasty, impressionable practitioners, exasperated by their fallibility, infatuated with novelty, enticed by example, and eager for local notoriety, have yielded to temptation, have risked numberless abdominal sections in the hope that chance would favour them, and so have helped to prove how strong is the contagion of folly.

When one recollects how many such cases undergo an unaccountable spontaneous cure, how often the symptoms cease after some mental impression or physical shock, or a perseverance in some extraordinary position, how many fruitful but painful ovaries have been saved by Dr. Weir Mitchell's systematic treatment, and how often it has happened that a threatened, simulated, or imperfect operation has been enough to frighten or charm away all acquaintance with suffering, doubt falls upon both the asserted necessity and the reputed success of the operation itself. It can never be determined how much is due to the amputation, how much is a psychical phenomenon. How many women have been doomed to sterility that would have been equally relieved by a farce or a failure can never be made out. But it is a query which takes the gloss off a mass of statistics.

Still we do not pretend to say that cases of nervous dysmenorrhœa, with neuralgic hysterical symptoms, are not occasionally to be met with in which medication fails, and for which there is no other alternative than operation or endurance. But we maintain with Battey, Duncan, and many other wise and prudent men, that such patients are comparatively few and far between. And it is only when one hears such well-authenticated, revolting stories as that of a young lady who, in good health, leaves her home, goes to London or some provincial town, happens to have a trifling indisposition,

consults —, who, troubling himself only to draw out the avowal that her periods were accompanied with *quelque malaise*, on the spot insists upon oöphorectomy, and a few days afterwards does it, that the mystery of some recent statistics is unveiled. Fortunately, she lived to return and tell what had passed. If she had died, what ought to have been the verdict? Have earlier or later warnings been ill-timed or impertinent?

No one can pique himself upon the outcome of oöphorectomy for mental alienation. A few melancholy girls, worn out by long suffering, and driven to think of the river by disappointment at the abortiveness of doctoring, may have laughed and found life tolerable afterwards; for women are not morally affected by castration in the same way as eunuchs. But pure madness—no; gynæcologists will never empty the lunatic asylums. They have sent some women into them. Madhouse dissections show generally only good or atrophied ovaries, and scarcely in any real ovariectomy cases have the patients been mad.

The pleasantries of men we do not care to name, who talk of freeing the world from the mad and the bad, only point to the extinction of the human race and the self-castration of the last man—for he who cuts mad people must himself be mad. And as for nymphomania, one operator put his own person in danger when he counselled so inconsequently. He must have known that passion in women mocks at oöphorectomy, and his illogical reasoning condemned his judgment. Call a spade a spade, and what would such oöphorectomists be?

The erotic fury and bad habits which mark the ill-regulated mind are matters more for the consideration of the moralist than the surgeon. Parents, tutors, and the clergy ought to be the guides and protectors of youth. The profession can only act by instructing them. No one has more wisely handled this subject than Wheelhouse, the worthy consultant of Leeds, nor can anyone do better than recommend his pamphlets and quote the concluding sentences of one of them:

‘If medical men will teach parents the true nature of the dangers to which, when they leave their fostering care, their children will be exposed—

‘If parents, acting on the knowledge thus imparted to them, will conscientiously fulfil their duty to their children—

‘If those to whom the education of the young is entrusted will see that ignorance is displaced in favour of wholesome knowledge—and that, as far as possible, purity of mind, as well as intellectual culture, are the objects at which they should aim, then I think all else will rest with the clergy—and that they, by their positive

teaching of the essential holiness of God, with the perfect, spotless character of Christ, as the revelation of the pure nature of the Deity—and as the measure of our own high calling, may rightly be left to inculcate these as the strongest of all moral antiseptics.’

The following are the conclusions that may be drawn from the facts of command :

That the operation of oöphorectomy, or the removal of normal ovaries, is one which may be advised in some cases of uterine fibroids, and in uncontrollable uterine hemorrhages.

That it is to be resorted to in certain malformations of the genital organs, deformities of the pelvis, and accidental obstructions of the vagina.

That the right to use it is very limited in cases of ovarian dysmenorrhœa or neuralgia, and only when they have resisted all treatment, and life or reason is endangered.

That in nearly all cases of nervous excitement and madness it is inadmissible.

That it should never be done without the consent of a sane patient, to whom its consequences have been explained.

That the excision of morbid ovaries and appendages should be distinguished from oöphorectomy, and ought not to be done without the authority of consultation, as in most other cases of abdominal section.

That in nymphomania and mental diseases it is, to say the least, unjustifiable.

Professional reputation is a sacred trust. Generations have handed down that of physic unsullied. To maintain it is with us a personal obligation, and our individual responsibility is now greater than ever. There is so little scope for concerted action in medicine, that the popular estimate of it must be an *ex pede* estimate. Medicine has none of the symbolical trappings of state authority or of celestial inspiration which tell upon the fears and superstition of the people. It is hope that makes them seek and cling to it as their first refuge in time of trouble. It is with them, in their homes, their guide and comforter. People pay little heed to presidents and rectors. The village doctor represents the profession to them, and each in his little circle is high priest and chancellor. By his skill and conduct the whole faculty is judged. He is like one of the fragments in a mosaic. If he is unsound, goes wrong, fails in fidelity to the *lex non scripta* of his class, he falls from his place, leaves a blot which all can see, and the whole composition is marred. In the exercise of his profession each member is so independent, while all are so linked together in honour and duty, that, as sentinels, we have a mutual interest in keeping watch and ward

over each other's loyalty, and sounding an alarm in case of default. Of late, the laparotomy epidemic has called for one of these challenges. It has roused a feeling not of jealousy, but of suspicion and concern for professional honour. When men in clubs begin to jeer at gynæcological domiciliary fussiness, and husbands are furious at the rumours of mysterious diseases, unknown to Sydenham and Cullen, being rife among their wives and daughters, there must be something wrong. It is time to look into the matter. If we hold the mirror up to Nature, only changing the sex of the actors, the spectacle is not flattering. Fancy the reflected picture of a coterie of the Marthas of the profession in conclave, promulgating the doctrine that most of the unmanageable maladies of men were to be traced to some morbid change in their genitals, founding societies for the discussion of them and hospitals for the cure of them, one of them sitting in her consultation chair, with her little stove by her side and her irons all hot, searing every man as he passed before her ; another gravely proposing to bring on the millennium by snuffing out the reproductive powers of all fools, lunatics, and criminals ; a third getting up and declaring that she found, at least, seven or eight of every ten men in her wards with some condition of his appendages which would prove to be incurable without surgical treatment, and a bevy of the younger disciples crowding around the confabulatory table with oblations of soup-platefuls of the said appendages ; if too, we saw, in this magic mirror, ignorant boys being castrated almost impromptu, hundreds of emasculated beings moping about and bemoaning their doltish credulity, showers of cases, ready for cutting, falling like manna, every morning, at one spot, while in another they drop in at the rate of one and a half the year—should we not, to our shame, see ourselves as others see us ? And if at the same time we were to hear a few of the sisterhood, more frightened than shocked, muttering remonstrances, and crying out, like the Ephesians of old, that their craft was in danger—say, should we not be bound to enter the strongest protest against their selfish wailings, and indignantly to denounce such follies as a personal degradation, a crime against society, and a dishonour to the profession ?

With the author's comments.

A PAPER
ON
KOCH'S
Researches on Tuberculosis,
AND
ABSTRACT OF A LECTURE
ON SOME OF THE
MORE RECENT FACTS AND OBSERVATIONS
CONCERNING THE
Bacillus of Tubercle.

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—
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KOCH'S RESEARCHES ON TUBERCULOSIS.

Read before the Glasgow Medico-Chirurgical Society, on December, 1st, 1882.

Deposited with the President of the Society, November 3rd, 1882.

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ON the 24th of March, 1882, in a paper read before the Physiological Society of Berlin, Dr. Robert Koch claimed to have established, by experiment and by observation, the existence of a micro-organism which is associated with tubercle, and not only associated with tubercle, but, according to Koch, the cause of all tubercle. This organism is a bacterium of the kind known as a bacillus, and it is, consequently, rod-shaped. In length it varies from about $\frac{1}{3000}$ to $\frac{1}{12000}$ in., and its breadth is about $\frac{1}{5}$ th of its length. In looking at a specimen of these bacilli, it will be seen that certain of them contain spores, two to four, ranged along the length of the organism.

In attempting to give an outline of some of the points on which Koch lays especial emphasis in the lecture referred to above, it is obvious that attention must be given solely to what is there stated. Since that date no observations have been published tending to disprove Dr. Koch's work. On the other hand, the bacillus described by him has been found by several observers in the tissues and in the sputa of persons whose conditions of disease would have suggested to any clinician, of ordinary experience, the probability of the presence of tubercle in the patient. It must, then, be admitted, that we have now to deal with a new fact which characterises, by the presence of these organisms, certain cases of disease of well known type, about the exact clinical significance of which there is, even now, no inconsiderable difference of opinion. For those who find themselves justified in accepting Koch's results as true, all difficulties about the nature of these cases must cease, as soon as it is found that the patients concerned harbour in their tissues, or in their secretions or excretions, this bacillus of tubercle.

The bacillus is demonstrated in tissues by employing the process first described by Professor Ehrlich. Koch has

adopted this process in preference to the one devised by himself, and with the aid of which he worked out all his early experiments. Ehrlich's process will be found fully described in the *British Medical Journal* of 14th October last, and Professor Vignal makes some useful remarks upon the process in the same *Journal* on 28th October. It is, for these reasons, unnecessary here to touch upon the method of investigation required for the detection of the bacillus. There is, however, one error in my remarks as they appear in the *Journal* of 14th October. I ought to have said, that the bacillus of leprosy gives precisely similar results with those shown by the bacillus of tubercle when these two organisms are submitted to the process of staining devised by Ehrlich. There are, however, some differences in form, as Dr. Koch points out, between the two bacilli. The bacillus of leprosy is "more slender and more pointed at the ends" than that of tubercle. They are also distinguished from one another by the colour test of Weigert, to which the bacilli of leprosy respond; those of tubercle, on the contrary, are unaffected by it.

Dr. Koch thus describes the appearance of the bacillus in tuberculous tissues:—In all cases where the tuberculous process is in its early stage and progressing rapidly, the bacilli are to be seen in great numbers. They then lie thickly, and often in groups or small bundles inside the cells, and in some places give the same appearances as the bacilli of leprosy when they are found in cells. Near these (groups or bundles) are found numerous free bacilli. Especially on the borders of large cheesy deposits crowds of bacilli appear, which are not shut up in cells."

"As soon as the highest point of the tubercle eruption is overstepped the bacilli become rarer, or are only to be found in little groups or singly at the edges of the tuberculous deposits, and lying near them are bacilli so faintly coloured as scarcely to be recognisable; these are presumably already dead or in the act of dying. Finally, they can quite disappear, although they are rarely altogether absent, and then only in such places as those in which the tuberculous process has come to a standstill."

In his lecture, Dr. Koch lays emphasis upon the connection which appears to exist between the presence of the bacillus and of the giant cell. "If," he says, "in the tuberculous texture giant cells appear, then the bacilli lie by preference in these structures. In cases of very slowly progressing tuberculous processes, the inside of those giant

cells is generally the only place where the bacilli are to be found."

Dr. Koch has a theory about the connection between the giant cell and the bacillus, and it is this:—"It is to be concluded from the size and position of the giant cells containing bacilli, that these cells are the youngest, while, on the other hand, those cells which are free from bacilli are the oldest, and it is to be assumed that these last originally contained bacilli, and that the organisms have either died or have gone over to that condition which will presently be described. From the observations of Weiss, Friedländer, and Laulamié, according to whom giant cells were formed around foreign bodies, such as vegetable fibres and the eggs of strongylus, we may be able by analogy to realise the relation of the giant cells to the bacilli. We may infer that here also the bacilli, as foreign bodies, are enclosed by the giant cells, and on this account, if the giant cells are found empty, all further relations of the tuberculous process go to shew that the presumption is correct, that the giant cells had formerly harboured one or more bacilli, the organisms having occasioned the origin of the cells." So much for the appearances described by Koch as illustrating the presence of the bacillus of tubercle in tissue, and its peculiarities there. It makes no difference whether the bacillus is seen in a human being or in a monkey, a guinea pig, a mouse, or a hen, the organism is always the same in every detail.

And now as to the facts upon which Koch, on 24th March last, founded his claim for the recognition of this organism as associated with tubercle. He found the bacillus present in the following cases:—

1st. In the human subject—

11 cases of miliary tubercle.

12 cases of cheesy bronchitis and pneumonia. (In six of these cavities had formed.)

1 case of tumour of brain of the size of a hazel nut.

2 cases of freshly extirpated scrofulous glands.

2 cases of synovial degeneration of joints.

Twenty-eight cases in all.

2nd. Amongst the lower animals—

10 cases of perlsucht of the ordinary type.

1 case of caseous cervical gland in a pig.

1 case of a hen which died of tubercle.

3 cases of spontaneous tubercle in apes.

9 cases of spontaneous tubercle in guinea pigs.

7 cases of spontaneous tubercle in rabbits.

Thirty-one cases in all.

“Besides these cases of spontaneous tubercle, I examined,” says Koch, “172 guinea pigs, 32 rabbits, and 5 cats, all of them infected with tubercle by the inoculation of the most varied tubercular substances, such as gray and calcified tubercle of human lung, phthisical sputum, tuberculous masses from spontaneously diseased monkeys, rabbits, and guinea pigs, pieces of lung from cattle suffering from perlsucht, cheesy as well as calcified, and, lastly, by inoculation from tubercular affections produced in animals by inoculation.” In each of these cases, 268 in all, bacilli were not once wanting, and in many instances they were extraordinarily numerous. So much, then, in proof of the statement that this particular bacillus is found associated with tubercle.

And now comes the second point. It remains still to indicate the line of evidence advanced by Koch in proof of the belief, that this bacillus, and nothing but this bacillus, is the cause of tubercle. To prove this, he carried out a series of experiments in which he took tuberculous particles from animals which had either died of tubercle, or, having tubercle, had been killed for experimental purposes. These particles were about the size of millet seeds, and were removed from the dead body and placed upon the blood serum, prepared in a certain way, of the ox and of the sheep, with scrupulous attention to all those precautions which are familiar, at least in theory, to every one who is acquainted with what are known as “cultivation experiments.” The object of these elaborate experiments was to obtain the bacillus of tubercle free from all taint. Dr. Koch believes that he succeeded in attaining this end. After describing how he sowed the tubercular morsels upon the prepared blood serum and watched their slow growth, and noted certain of its peculiarities, he makes a statement which deserves special attention. He says, “The extremely slow growth, which alone is to be obtained at breeding temperature, and the peculiar shovel-shaped, dry, and firm condition of these colonies of bacilli, are not to be found in connection with any other known bacterium, so that, the confounding of the culture of the tubercle bacillus with that of any other bacterium is impossible; and already, with only short experience, nothing is easier than to recognise at once accidental contamination of the culture.” This is a very important statement, and it is all the more important when it is made by Koch, one of our best experimenters and observers, and one whose words carry with them that

authority which can be given only to a profound and extensive knowledge, such as his, of the life history of bacteria.

After describing the appearances of the growth of the bacillus under cultivation, he says, "By the help of a low power (30 to 40 diam.) the colonies of bacilli" (undergoing cultivation) "are already visible towards the end of the first week. They appear as very elegant spindle-shaped and S-shaped structures, and also in other similar crooked figures, which, if they are spread out on a cover glass, coloured" (*i.e.*, submitted to Ehrlich's colour test), "and examined with a high power, consist solely of the familiar extremely delicate bacilli." Had any other known organism been present it is hardly within the bounds of possibility that Koch could have failed to observe it, for upon the accuracy of this observation hinges much of the worth of his researches into the nature of tubercle.

These cultivation experiments were carried on for some time. After from ten to fourteen days' cultivation in one test tube containing the prepared blood serum, some of the crop of the bacilli which had grown there was transplanted to another test tube, and after another ten days or so, some of this second crop was sown in a third test tube, and so on, until, in one mentioned instance, the cultivation extended to 178 days.

On such observations and experiments as these rests, in part, the proof that the bacillus of tubercle was obtained free from taint.

Dr. Koch next proceeded to inoculate certain animals with the pure bacillus, obtained by cultivation. The inoculation was always performed with every care against the possibility of contamination. In each series of experiments several animals were used, including rats, mice, guinea pigs, rabbits, a marmot, pigeons, frogs, &c. Some of these animals are, it is well known, difficult to infect with tubercle—a fact which is not without some significance in this connection.

Koch thus sums up the results of these inoculations:—If one looks back upon these experiments, one sees that a not inconsiderable number of animals were experimented upon, on which bacillus culture was brought to bear in very different ways—viz., through simple inoculation into the subcutaneous cellular tissue, through injection into the abdomen or into the anterior chamber of the eye, or direct into the blood stream, without failing, even in one single instance, to develop tubercle; and there had formed in them not solitary nodules, but an extraordinary mass of

tubercle corresponding with the large number of infecting germs introduced." In each of these series of experiments—there were thirteen in all—a certain number of animals were not submitted to the inoculation of the cultivated bacilli. These animals had been bought at the same time, and fed and lodged in the same way and in the same places with those animals which were inoculated with the bacilli, but not one of the former showed any evidence of tubercle, either during life or after they had been killed and examined, *post-mortem*. It must be remembered that these cultivation experiments were made with tubercle taken from the lungs, calcified mesenteric glands, and freshly extirpated scrofulous glands of human subjects, as well as from the lower animals, and that there was no difference whatever in the effects produced by inoculating from these two distinct sources; and the bacilli from these two sources were also identical in appearance.

Dr. Koch makes some very interesting observations about certain distinctions which he draws between tubercle occurring spontaneously in an animal and that type of tubercle which results from inoculation. He bought and examined one hundred guinea pigs, all of which were quite healthy. Several of them were shut up in a room with other guinea pigs which had been inoculated with the tubercle virus. In three or four months, but never before the lapse of that time, spontaneous tubercle began to show itself, and always sporadically, amongst the uninoculated guinea pigs. In them the bronchial glands were "always found unusually large and purulent, particularly, also, in the lung was to be found a large cheesy mass, with very far advanced breaking down in the centre, so that, sometimes, as in human beings, it had reached to actual cavity. The development of tubercle in the organs of the lower part of the body was very far behind that in the lung. The swelling of the bronchial glands, and the commencement of the development of tubercle in the organs of breathing leave it beyond a doubt, that the tubercle of these animals was an inhalation tubercle springing from a few or possibly only one infectious germ, and on that account very slow in its progress."

Contrast that description with what Koch says about inoculated tubercle, and the contrast will be found to be very suggestive. "The place of inoculation was in the belly of the animal, near the inguinal glands." The first sign of the success of the inoculation was the appearance, at the

end of a week, of a nodule over the site of the puncture. About the end of the second week, the inguinal glands beside the wound began to swell, and sometimes also the axillary glands. From that time the animals grew quickly thinner and died in from four to six weeks, with marked tubercular affection of the liver and spleen, those organs having been but slightly affected, as compared with the lungs, in the cases of spontaneous tubercle.

Several animals were inoculated with certain substances which did not contain the bacillus of tubercle; for example, morsels of a scrofulous gland, of degenerated synovial membrane from a joint, of a portion of monkey's lung kept dry for two months, of another portion of the same lung which had been kept in alcohol for one month, and in not one instance did the animals experimented upon with these substances shew any sign of tubercle either during life or *post-mortem*.

Several experiments with sputum from tuberculous individuals are mentioned in Koch's lecture. The sputum was allowed to dry, as it may sometimes be seen drying on a hospital floor, not always in an out of the way corner. Tubercular sputum, dried in this way, was found to be as surely fatal in its results, when an animal was inoculated with it, as had been the case when the cultivated bacillus was used. The specimens of sputa with which these experiments were made, were from two to eight weeks old.

A highly suggestive series of experiments were performed, with the view of ascertaining the effects of varying quantities of bacilli upon animals, into which the organisms were introduced by injection. The anterior chamber of the eye was selected as the site of the experiments. In one case the pure prepared blood serum, used in the cultivation of the bacilli, was injected. It was, however, in this instance, unmixed with the bacillus or any other organism. It was injected; and the animal was killed and examined thirty days after the operation. All its organs were found healthy. No bacilli of tubercle were seen although they were carefully sought for. In another case the injection was made with blood serum mixed with bacilli, which had been cultivated during 132 days. The needle of the syringe was pushed into the anterior chamber of the eye, but the piston of the instrument was not moved. In this way only an extremely small number of the bacilli could have entered the eye. In a fortnight from the day of the puncture, solitary nodules, of a light golden tint, appeared upon the iris near

the site of puncture. From that time tubercular iritis was developed, and the cornea became cloudy. In thirty days the animal was killed, and, besides the changes in the eye, the glands near the jaw and at the root of the ear were swollen and contained yellowish-white deposits. In two other cases the injection was made with blood serum charged with cultivated bacilli; but many drops were introduced into the anterior chamber of the eye. These two animals also developed the local symptoms indicated in the last case, and they rapidly became thin. In thirty days they were killed, and, in addition to the local changes, their lungs contained "innumerable tubercles." The lungs of the animal subjected to the inoculation of a minute portion of the blood serum free from bacilli were free from all sign of tubercle, and so were its other organs.

Experiments exactly similar to the foregoing were repeated again and again, and invariably with like results.

Koch thus begins to sum up and give what he regards as the outcome of his work. He says—"All these facts taken together justify the conclusion that the bacilli present in tubercular substances are not merely the associates of the tubercular process, but the cause of it, and that we have before us, in bacilli, the actual tubercle virus. It is also possible, by this means, to draw the boundary of those diseases regarded as tubercular, which, hitherto, could not be done with certainty. A decided test for tubercle is wanting, and one man considers miliary tubercle, phthisis, scrofula, perlsucht, &c., to be tubercle; another man holds, perhaps with equal right, that all these processes of disease are different. In the future, it will not be difficult to decide what is tuberculous and what is not tuberculous. Not the peculiar structure of tubercle, not its non-vascularity, not the presence of giant cells will decide the question, but the presence of tubercle bacilli—be it in the tissues by the colour test, or be it through culture on prepared blood serum. This criterion, taken as a guide, must, according to my researches, stamp miliary tubercle, cheesy pneumonia, cheesy bronchitis, tubercle of glands and of the intestine, perlsucht in cattle, inoculated and spontaneous tubercle as identical."

Many points of great interest have not been touched upon in this attempt to summarise Dr. Koch's work on tubercle, which received from him, in his lecture, considerable attention. It is hoped, however, that enough has been said to indicate the lines on which this admirable piece of

work has been founded and built. In thinking over it, many questions will suggest themselves to the mind, and answers will be sought for them. If these answers rest upon facts, then only good can follow when medical men come to consider the questions and their answers. If, however, mere personal opinion, founded upon merely personal bias, takes the place of facts, then we shall have discussions which will settle nothing and end nowhere. One question can hardly fail to be amongst the first to suggest itself—How does all this work of Koch's fit in with the hereditary nature of tubercular phthisis? If there has been any fact established in medicine by the evidence of patients, and the practically unanimous opinion of physicians of all countries, it is certain that the hereditary tendency of tubercle has been so established. Of late, however, in Germany, in France, and in America, as well as in our own country, some men have expressed doubts as to the soundness of the evidence upon which rests the all but universal belief in the heredity of tubercle. Of course, this is a perfectly legitimate position, and besides that, it has been taken by men, some of whom are entitled, from their clinical experience, to speak with authority upon the subject. Others have gone still further, and have denied the truth of the hereditary tendency of tubercular phthisis altogether. Koch does not fail to touch upon this point in his lecture. He takes up no definite position in connection with it, but simply indicates the opinion that we must have still further inquiry into this question. I think that, with his views, such an opinion is what might be expected from the man who has done such work as his.

We cannot yet tell what light will be thrown upon disease by Koch's great discovery. I call it a discovery, because whatever may be our attitude with regard to the bacillus of tubercle, it is true that Koch has shown us, in the presence of the bacillus in a certain type of disease, something which we did not know before.

It is only seven months since this discovery was given to the medical world. No doubt, many men have been engaged, since last March, in studying the clinical bearing of the tubercle bacillus. To speak about establishing clinical facts concerning a subject such as this in seven months, would surely be to misuse words. I have, however, had the advantage not only of observing my own cases, but also of receiving from Dr. Bristowe a short statement of some of his observations in his wards in St. Thomas's Hospital,

in this city. In a letter which I received from Dr. Bristowe, dated 22nd October last, and from which he kindly allows me to quote, he says, after detailing some cases under his care in which the bacillus had been found, "My experience, as you see, is limited; but so far as it goes it confirms the belief that a special form of bacillus exists in tubercle, and may be found in the sputa of phthisical patients. It tends, also to show that the examination of the sputa for bacilli is an important method of determining, in doubtful cases, whether we have tubercular or some other disease to deal with. My own experience does not tell me whether the bacilli are chiefly abundant in the sputa, in cases in which the lungs are breaking down. But I may observe that all the phthisis cases examined by me were well marked cases, and probably all had excavations."

At my request Dr. Lawrence Humphry, formerly Resident Medical Officer at the City of London Hospital for Diseases of the Chest, now resident at Cambridge, made a short statement of the results of his observations in connection with the tubercle bacillus in the hospital. It is as follows:—

"I.—*Cases of advanced phthisis with high temperature, &c.*—The sputum, in all cases examined, was highly charged with bacilli.

"*Post-mortem Examination.*—The fluid from the cavities in the lungs, the scrapings of the caseous nodules from different parts of the lung, the caseous parts of the bronchial glands, and in one case the mesenteric glands, contained them in large numbers. I did not find them in the tubercular ulcers of the intestines.

"II.—*Cases of acute secondary tuberculosis.*—In these the sputum contained a larger quantity of bacilli than in any other cases, as did also the fluids from different parts of the lungs. Two or three of them were remarkable for their acute progress and rapid termination.

"III.—*Cases of incipient phthisis.*—In most bacilli were found in small numbers, one or two in a field. In some no bacilli were found after repeated examinations.

"IV.—*Bronchitis, subacute, chronic, and asthmatic.*—No bacilli in the sputum.

"V.—*Chronic fibroid.*—In cases complicated with caseous pneumonia or secondary infective processes, the sputum contained bacilli; also the fluid of the lung cavities."

That is what Dr. Humphrey has to say on this subject. I know that he has taken great care to make his obser-

vations exact, and I am under a debt of gratitude to him for his kindness in supplying me with material for my own work, even, as sometimes happened, at considerable inconvenience to himself.

In the beginning of October I wrote to my friend Dr. Koch, laying before him some of the impressions made upon me by what I had myself seen in practice since last June, when I began to follow this line of observation. I wished to know what he thought about certain points which seemed to me to be worthy of attention. Dr. Koch is not himself in practice, and, therefore, will give no opinion as to clinical matters. He told me, however, in reply to my letter, that the tendency amongst those hospital physicians with whom he had conversed on this subject, was in the direction of the belief that, in the future, the detection of the presence of bacilli will be of more importance than physical diagnosis, because it is a sure sign of the presence of tubercular phthisis.

As regards my own observations:—I have notes of 54 cases, and I hope, by and bye, to publish details of these and of other cases still to be observed. For the present, however, I think that my experience is far too limited for me to speak about it at any length. I will, however, venture to indicate what seems to me to be the tendency of the short clinical experience I have had. I think that we have now a method of investigating lung disease which, standing alone and unsupported by any other method of examination, throws a special light upon a patient's condition. This much I can already say, speaking from my own experience, that bacilli of tubercle are not always present in the sputa of patients whose physical condition would lead anyone who knows something of Dr. Koch's work to expect to find them there. It is also a matter within my own experience, and it has happened to me more than once, to search the sputa for bacilli of tubercle and to fail to find them early in the history of a case of consumption. In the same case, within one or two months (for, as I have said, there were more than one such case) I have found bacilli in the sputum, and in one instance in enormous numbers.

My experience also inclines me to expect to find it established very shortly, that in the prognosis of phthisis we must look to this method of investigating the sputum for valuable information. I think it will be established that, given persistence of a large number of bacilli of tubercle in the sputum early in the history of a case, and that case will

run a short course and end in death. On the other hand, I think it will also be established that, given few bacilli of tubercle in the sputum of a consumptive, and given also, that that condition of fewness of bacilli in the sputum characterises the case for some weeks, then that case will probably run a long course.*

ABSTRACT OF A LECTURE ON SOME OF THE MORE RECENT FACTS AND OBSERVA- TIONS CONCERNING THE BACILLUS OF TUBERCLE.

Delivered at the City of London Hospital for Diseases of the Chest,
March 20th, 1883.

[Reprinted from the "*British Medical Journal*" for April 28th, 1883.]

GENTLEMEN,—As you know, I have undertaken to address you to-day in response to a request made to me by some of your number. I have been asked to speak to you about a part of the work that has been published concerning the bacillus of tubercle. To give you anything like a full review of this subject in one lecture, would be a task which I do not think anyone at all conversant with its literature would attempt. For that reason I asked that I might be told upon what points you wished me to touch, and, in reply to that request, I have received an answer which shews me the line you wish me to follow.

You are, of course, all aware, that the bacillus of tubercle was discovered by Dr. Robert Koch of Berlin. He made his discovery known to the medical world in a lecture, given in Berlin, on March 24th, 1882. As a first step in what I

* Since this paper left my hands, views similar to those indicated in the text have been published by Drs. Balmer and Fraentzel in the *Berliner Klin. Wochenschr.*, No. 45, 1882, so that their observations and mine appear, in some degree to confirm each other. They speak, however, from a more extensive experience than I then had, and their observations go farther than mine.—G. A. HERON.

have to say to you to-day, I shall endeavour to indicate in as few words as I can, an outline of portions of Koch's work. I must confine my remarks to what seem to me to be some of the more important points, which his researches and observations have brought so prominently before students of medicine of all conditions. My object in doing this, is to endeavour to bring clearly before your minds the source whence Koch obtained the bacillus of tubercle. It was whilst searching tuberculous tissues with the view of ascertaining, if possible, what that condition meant, that Koch discovered that a certain rod-shaped bacterium was present, sometimes in enormous numbers, but, also, sometimes in very small numbers, in all tuberculosis tissues. This rod-shaped bacterium, the bacillus of tubercle as it is called, is now familiar to you all. Koch discovered this organism by submitting those diseased tissues to the action of a certain staining fluid, devised by himself. Here I come to a part of the subject on which you have asked me to speak at some length, for you want to hear about the staining of the bacillus. I need not say more about Koch's own method, for he gave it up when Professor Ehrlich told him of another method, which he had discovered, and Koch at once adopted it and has used it ever since. There is, so far as I know, no other method but Ehrlich's now in use for staining the bacillus of tubercle. Various modifications of his process have been suggested; but, in all points of importance, it remains as it was given to us by Ehrlich himself. The modifications which are improvements of this process, aim at giving the staining mixture a uniform composition; and several different formulæ have been suggested for that purpose. The one now in use in this hospital, and I have myself used it for some months past, is Weigert's. Its composition is, saturated alcoholic solution of fuchsin, or methylated violet, or gentian violet, 11 parts; anilin water, 100 parts. (The preparation of the anilin water, and the use of the staining fluids, were described by Dr. Heron in detail).

In warm weather good results are to be obtained when the staining process is carried out at summer temperature, and without the use of artificial heat. I should advise you, however, always to stain your specimens while they are exposed, in an incubator of some kind, to a temperature of 98° to 100° Fahr. Want of attention on my part to this important point caused me much inconvenience when the cold weather set in last autumn.

The process I have just described to you occupies some time. It has, however, certain advantages. Not only does it ensure thoroughly good staining of the bacilli, but the specimens, while being stained, may be safely left from half an hour to ten hours, or even longer, in the fuchsin and anilin mixture, just as the experimenter finds most convenient. There is, however, a more rapid way of staining. The same staining fluids are used. A little of the fuchsin dye is filtered into a watch glass; the specimen to be stained is placed in the fluid, and heat is applied. I have been accustomed to apply the heat by lighting a Bunsen burner, not at the top of the funnel, but at the burner itself. The heat, of course, passes up the Bunsen funnel, and so reaches the watch glass, placed at a convenient height upon a tripod. I find that one minute's exposure to the action of the staining fluid, under these conditions is sufficient to ensure excellent colouring of bacilli in sputum or in pus. The rest of the process is identical with what I have already described to you. Following this plan, it is easy to stain and examine with the microscope, a specimen of sputum within ten minutes' time. Most of the specimens of the tubercle-bacillus in sputum and in pus which you see here to-day, have been prepared in this rapid way. In speaking of these staining fluids I have called the red dye fuchsin. That is the name in use on the Continent for the colouring matter which in England we call magenta.

You must not, however, overrate the importance of this colour test. It is not on such a comparatively trivial observation as that that Koch has founded his claim for the special recognition of this organism. Were it shown to-morrow that half a dozen other bacilli, alike in appearance with the tubercle-bacillus, behave in presence of dyes as it does, Koch's reasoning and conclusions concerning this organism would in no way be effected. They are founded upon his cultivation experiments and their outcome, and not upon a mere colour test.

Now, let us turn again to Koch's early researches. Having examined the organs of many animals, including men, known to have died of tubercle, or which, having tubercle, had been killed for experimental purposes, and having found that the bacterium which we now call the bacillus of tubercle was invariably present in greater or less numbers, Koch came to the conclusion that this organism is the constant associate of the tuberculous process. He next set before himself the task of endeavouring to ascertain the

exact relationship of the bacillus to the tuberculous process. With that object in view, he began a series of cultivation experiments. He sowed some tuberculous tissue in a little of the blood-serum, prepared in a certain way, of the ox and of the sheep. The tuberculous morsels thus sown were removed, with every possible precaution against contamination, from the bodies of animals, man included, dead of tubercle. It would be easy to occupy an hour of your time in trying to put before you an outline of Koch's experiments and their results. I can only now state to you, that Koch believes that he has proved that this bacillus of tubercle is not only the associate of tubercle, but that it is the actual virus of tubercle. The last of these two conclusions is, of course, of vast importance when we find it accompanied by the evidence which Koch advances in its support. That evidence is the outcome of the cultivation experiments to which I have just referred. By their means, Koch was able to grow the bacillus of tubercle on the blood-serum of oxen and sheep; and he satisfied himself, by most careful observation, that no known organism was present besides this bacillus. Koch next proceeded to experiment upon animals with this pure bacillus. Every care was taken to insure the introduction of the bacillus of tubercle, and of that organism alone, into the bodies of the animals used. In every instance—and some hundreds of animals were thus experimented upon—tubercular disease followed the injection of the bacillus of tubercle into the animal's body; and, in every case, the bacillus was found in the tuberculous organs of these animals when they were examined after death. The organism injected into these animals, and the organism found in their bodies after death, were identical in appearance; and the bacilli remained the same in appearance, and they retained, apparently, the same virulence, no matter how often they were made to pass from one animal to another.

Gentlemen, that is a very meagre outline of a part of Koch's work. I hope, however, that I have said enough to show you that his work was done with the most laborious care, and that it cannot be set aside by anything short of experimental demonstration of error in its details. Only three days ago, the medical weekly papers contained an abstract of an elaborate series of experiments by Mr. Watson Cheyne, which he has carried out upon the lines laid down by Koch. These experiments and observations confirm Koch's work on tubercle, and even carry the subject beyond the point at which he left it.

Few subjects have raised more discussion amongst medical men than that one which is introduced by the question, "What is tubercle?" Of the many answers which this question has received, probably none has attracted more widespread attention than that given to it by Dr. Koch's researches. Those of us who accept his teaching as true, must look upon every case in which the bacillus of tubercle is found, as tuberculous. During the last twelve months, there has been gradually accumulating a mass of evidence in favour of the view that this bacillus is the constant associate of tubercle. This evidence comes from observers in the Old World and in the New. It comes from men who work in the dead-rooms of hospitals, as well as from those whose observations are made at the bedsides of the sick; and this evidence is already so strong, that it seems to me we are justified in stating it to be a fact, that wherever tubercle is, there the bacillus of tubercle is also. Whether this organism is the cause of tubercle is another question; and it is not to be expected that men who have spent long years in the study of tubercle, and who have their own views about tubercle, will readily accept Koch's teaching as all true—unless, indeed, their own observations have already prepared them for the acceptance of such views. Of one thing we may feel sure, that the subject will not rest where it is, and that every week will add more and more to the weight of the evidence which must, probably very soon, definitively settle the important question—Has Koch discovered the cause of tubercle? Should that question be answered in the affirmative, then every case in which the bacillus of tubercle is found, must be classed under the head of parasitic diseases; and all discussion as to whether miliary tubercle, chronic phthisis, scrofula, perlsucht, &c., are tubercle, or whether each of them is a distinct disease, must end with the establishment of the fact that they are all due to the parasite which Koch has discovered.

And now, gentlemen, let us turn to another part of the subject. Not only is the bacillus of tubercle found in the dead body, but it is also easily obtainable from certain cases of disease in the living man. It has been found in the breath and in the sputa of persons suffering from tubercular disease implicating the lungs; it has been found in the urine in cases of tubercular disease of the kidney; it has been found in the fæces in cases of tubercular ulceration of the intestines, in that connection enabling a diagnosis to be made between tubercular and amyloid disease, in one

published case where, excepting the diarrhœa, the symptoms were not well marked. Lichtheim has recorded the case of a patient who presented the symptoms of acute pneumonia, and in whose sputa the tubercle-bacillus was found. This organism has also been observed in cases of lupus, in freshly-opened scrofulous glands, in synovial degeneration of joints, in the meninges of the brain, in an ulcer of the tongue, and in a previously unopened suppurating knee-joint. These examples suffice for our present purpose; and it is obvious of what vast importance it must be to ascertain the presence or absence of the bacillus of tubercle in such cases. But you have asked me to refer to those cases of lung disease in which we may expect to find the bacillus present in the sputum. The evidence in favour of the belief that this organism is to be found in the sputum of, practically, every case of consumption is now so strong, that I feel warranted in speaking of it as a fact. I have myself found the bacillus in the sputa of one hundred and sixteen patients, all of whom presented, sooner or later, what I regarded as unmistakeable signs of consumption. In the majority of these cases, there could have been no reasonable doubt as to the diagnosis of the disease, for the physical signs of pulmonary phthisis were in them very plainly marked. Now and then, however, I chanced upon a case in which the physical signs were so slight, that I was unable to form a decided opinion about the patient's condition and prospects, until an examination of his sputum showed me that it contained the bacillus of tubercle. With that knowledge in my possession, I cannot now doubt about a patient's condition, for I can but regard the case as one of tubercular disease of the lungs. In certain of these cases, too, an examination of the chest yielded no positive information beyond the fact that the patient was apparently suffering from a pretty sharp attack of bronchitis. The patient's history, and some dulness, more or less marked, over the apex or apices of the lungs, usually suggested to me the probability of the presence of pulmonary phthisis in the majority of such cases; but it happened more than once, that an examination of the sputum and the discovery there of the bacillus of tubercle formed the point in the case which fixed for me its diagnosis.

You all know how difficult it often is to distinguish, with certainty, some cases of bronchiectasis from certain cases of phthisis with lung excavation. An examination of the sputum, by Ehrlich's process, will surely fix the identity

of such cases ; and the same remark applies to some cases of empyema. "Is this patient tuberculous?" is a question often asked in such cases, and it used to be by no means always an easy question to answer.

This method of diagnosis is often, as I have already told you, of great use in cases where the physical signs are very indefinite. Litchtheim, Heller, and others, have recorded instances in which, though they could detect no physical signs indicative of disease of the lungs, the bacillus of tubercle was found in the sputum. If the time at our disposal permitted of it, I should like to dwell at some little length upon such cases as these, for they suggest many questions of interest. We must, however, pass on ; and I will only now say, upon this part of the subject, that I have not yet seen a case with absolutely no physical signs of lung mischief, in which I have detected the bacillus of tubercle in the sputum. Sputum has been sent to me for examination, taken, I was told, from a patient who, at that time, showed no symptoms of lung-disease, and in it there were tubercle-bacilli. Later on, signs of lung-phthisis became well-marked in the man's chest, and, *post mortem*, extensive tubercular disease was found to have existed. This patient was under the care of two able physicians, who, I hear, intend to publish the case.

Some of you, I think, were present at a lecture I gave, at the London Hospital, last October, at Dr. Andrew Clark's request. You may remember that I then said, that it seemed to me to be probable, that the presence of the bacillus of tubercle in the sputum would be found useful in prognosis as well as in diagnosis. I was speaking then from my observation of fifty-four cases, in whose sputa I had found the organism ; and I said that my experience led me to incline to the belief, that the persistence of a large number of these bacteria in the sputa of a patient indicates a case which will run a rapid course ; and that a persistence of few bacilli in the sputa would probably come to be regarded as indicative of a very chronic case. In the end of November, Drs. Balmer and Fräntzel published similar views concerning the prognostic value of the bacillus. They had then found the organism in the sputa of one hundred and twenty patients, so that they spoke from a much larger experience than mine. Crämer, and several others, have recorded like conclusions. On the other hand, Lichtheim of Berne, a very careful observer, though he does not distinctly differ from these views about the prognostic

value of the bacillus in cases of phthisis, is doubtful of their correctness. In certain instances of rapidly fatal phthisis, you will find that the sputum is apt to contain little clumps or groups of the bacilli, surrounded by large numbers of these organisms. When this grouping of the bacilli into little masses persistently characterises the specimens of any sputa which you examine, you will, I believe, find that you have to deal with a case which will run a remarkably rapid course, from the time of the appearance of this persistent grouping of the bacilli. My attention was first called to this grouping of the organism by several cases which came under my notice in this hospital, in the summer of last year. Since last November, I have seen only two cases where this peculiarity was persistently present in the sputum. Also, no other observer, so far as I know, has mentioned this point; so that it is, probably, not a common occurrence. All the cases, however, in which I have seen it, have run a very rapid course to death.

And now, let us give a very short time to summarising the results of observations bearing upon the habitat of the bacillus of tubercle in the lungs of the human subject. In cases of phthisis, it is found in the lining of the walls of lung-cavities, and in and around caseous masses scattered here and there throughout the lung, usually with some evident relation to centres of active disease, but sometimes, apparently, more or less isolated. The organism is not found to infest the lung-tissue itself in great numbers, and you may examine section after section of lung without finding a single bacillus. As a rule, all these centres of bacillus-life communicate with the air tubes; but to that rule there seem to be marked exceptions. That bacillus life may flourish when entirely cut off from all communication with the external air, is a fact which is demonstrated by some of the cases alluded to at the beginning of this lecture.

I cannot do better than give you, in brief, what Mr. Watson Cheyne has said about the appearances in the lung which go along with, and result from phthisis. We have here, under the microscope, a few specimens illustrating some of his results in this line of research. Those specimens have been prepared by Mr. Watson Cheyne, and he has most kindly lent them to me for your inspection (A short *résumé* of Mr. Watson Cheyne's recent work and views in relation to phthisis was then given.)

And now, gentlemen, I must bring these remarks to a close. No one can be more conscious than I am how im-

possible it is to give, within an hour's time, more than a mere summary of even those points which have occupied our attention to-day. We must leave almost untouched the burning question, for it has now assumed that shape—"Is phthisis an infectious disease?" The consequences involved in an answer to that question are so grave, that every piece of evidence tending to settle it, one way or the other, must be looked at with the closest scrutiny. There are not wanting records of observations tending to answer the question in the affirmative, and, certainly, there is no lack of evidence which is regarded by many as proving that phthisis is not infectious. It seems to be established as true, that this disease does not readily pass from man to man, under the ordinary conditions of life. All our clinical experience goes to prove the truth of that belief. But it does not follow from this that there are not men who possess, in some peculiarity of their physical life, a factor which, under certain conditions, may favour in them the development of phthisis. If that be true, it would follow that at least two factors are necessary to the production of phthisis; one peculiar to the individual man, and, therefore, always present with him in possibly a varying degree of intensity, the other, coming from without, and probably present only under certain conditions. According to the view which now seems to be gaining most adherents, the former of these two factors would be what constitutes the hereditary tendency to phthisis. What physical condition these words imply, is to-day an unsolved problem. Of the second factor, that one which comes from without the body, we are surely amply justified in asking: Is it not this bacillus of tubercle which Koch has discovered? Whatever may be our answer to that question, if it be admitted that two such factors exist and are necessary to the development of phthisis, then it seems to me, that we cannot shut out the conclusion, that for a large proportion of mankind, phthisis is an infectious disease.



C. t. 16

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With the author's consent

C. t. 16

A

History of Syphilis:

REPRINTED AND ENLARGED

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BY

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HISTORY OF SYPHILIS.

Was Syphilis known to the Ancients ?—It can scarcely be said that the history of this disease is in a satisfactory state. The same observation might indeed be made with regard to many other affections, but it seems particularly hard to get up any enthusiasm about the origin and extension of syphilis ; partly, perhaps, because the story has been so often told ; partly, it may be conjectured, because it is disfigured with fables which the most cursory examination leads us to reject, and encumbered with evidence which rather overloads than strengthens the argument, and obscures the subject which it was designed to illustrate. And thus, though successive authors have handled the topic with care and erudition, I have seen reason to think that this part of their works often awakens less interest than any other. Indeed some of the most recent writers show, by their brief notices or utter silence, either that they do not think the matter worth spending time upon, or that they see no ground for giving up long-received opinions, while the great enigma of the single or double nature of syphilis, so far as it is involved in the history of the disease, can scarcely be looked upon as settled to the general satisfaction of readers.

And to speak plainly, the details are so dry, the quotations from old authors so repulsive in their very look, and often so doubtful as to the evidence they afford, that genius has not been able to lend them interest ; nor can any expenditure of toil, any amount of polishing, communicate vitality to such a mass. It seems to defy even the ability of such critics as Haeser and Chaballier, and I believe we might almost as well try to call up the dead men from their graves, and ask them to tell us what they knew about syphilis, as to galvanize these fragments into life and shape. I see therefore no way but to pick out and adapt only what seems to be the best part of the evidence, and for this purpose it will be requisite to apply the pruning-knife so freely that

possibly enough the reader may think it has been handled with more vigour than discretion. But there is no help. To make a paper on such a subject readable, to make the chain of argument at once manageable and strong, the evidence must be thoroughly sifted, the only question being how far rejection shall go. Besides, what cannot be proved except by means of such encumbrances is not established any better by their aid. That such is practically, if not theoretically, the case, is, I submit, shown by the fact that, notwithstanding the pains taken by a few authors to uphold the antiquity of syphilis, the old belief is still the prevailing one; for if men really entertained even a suspicion that the complaint might be ancient, whence comes this constant repetition of the cry about the great severity of syphilis at its first outbreak in 1494, seeing that by no possibility can both views be right?

I propose to begin by cutting out all reference to Scripture. If the evidence of the hebrew writ could be relied on to turn the scale, I should hold myself quite justified in employing it. But it is not so. There is no reason why syphilis should not have prevailed, as some writers maintain it did, among the old Israelites, but I do not see any chance of getting beyond mere probability, and we want something more than that; the proof must be of a nature to satisfy, not only the historian but the pathologist, and this we cannot expect to find in a work which was clearly never intended to teach men science. For these reasons I feel no hesitation about excluding quotations which, while they bring us no nearer the proposed goal of our inquiries, are calculated to shock feelings which all men should respect.

In the same way I would deal with the everlasting quotation from Herodotus about the disease inflicted upon the Scythians, which no man has yet made out, but which assuredly was not gonorrhœa as has been surmised; for the old historian says,¹ that those who come into the country of the Scythians may see (*ὁρᾶν*) in what manner these people are afflicted, which could only apply to a disease producing visible effects; besides, it is hardly probable that even rude tribes would ascribe gonorrhœa to descent, and the Scythians believed the malady in question to be hereditary. Those writers who have surmised that Herodotus alluded

¹ *Clio*, 105.

to a disgraceful form of effeminacy, the victims of which were in the habit of dressing themselves like women, may very likely be right, but really what Laurent tells us about the greek adjective used by Herodotus to designate the nature of the affection, renders only too probable the conjecture that its meaning is as completely lost as the use of the *kist-vaen* and *cromlech*. I must therefore conclude that to translate *θήλειαν νόσον*, or any part of the passage, by “a running from the penis,” as M. Chaballier has done,¹ is something more than a free interpretation. Along with the narrative given by the great father of history, may be dismissed the allusion in Horace to the disease under which Cleopatra’s troop suffered, being simply one of those perplexing uncertainties which are far too vague for the purposes of any writer who aims at being accurate, Juvenal’s mention of warts and condylomata, and all similar evidence drawn from the roman essayists and poets.

The local Disease known to them.—So far as I can depend upon my own judgment, I should say that the question at the head of this paper must be answered in the affirmative, and that the description given by Celsus,² of ulcers on the glans and foreskin, sometimes dry or with little secretion, sometimes phagedænic, &c., indubitably applies to syphilis, and is the first clear and unimpeachable account that we have of the primary disease. To reproduce the description would lead to a diffuseness foreign to the object of this paper, but I quite concur in what Adams, an author not at all likely to indulge in any fanciful surmises about the antiquity of syphilis, says. He points out³ that we have here every form of primary sore typified except the true hunterian chancre; while the sloughing phagedæna, or nigrities serpens, of the roman surgeon, is the same affection as that described by Abernethy.⁴

We are here then face to face with a fact which can neither be ignored nor explained away. It is true that from this time onwards, for generations, we meet with but few clear traces of

¹ *Thèse pour le Doctorat en Médecine*; 1860, p. 12.

² *Liber vi*, Cap. 18, 24-5-6-7.

³ *Observations on Morbid Poisons*; 1807, p. 242.

⁴ *Ibid.*, p. 229.

syphilis, while at last even these seem to be lost in the works of the arabian writers. Such relics as there are may be found carefully detailed in the writings of Haeser, Chabulier, Astruc and others. I content myself with referring to two which will serve the purpose in hand, that of showing the continued occasional notice of primary sore. The first of these is an unmistakable fact mentioned by M. Robert, who says¹ that M. Becquerel describes, from the account given by an antiquary, certain votive tablets found in an old temple near the Seine, which contained sketches of phagedænic ulceration of the genital organs and of buboes, subjects not likely to have suggested themselves to the imagination without there had been a foundation of fact; indeed I cannot conceive anyone thinking of such a thing unless he were familiar with the occurrence. I searched for the original memoir according to the reference given by M. Robert, but could not find it, so that my account is at second hand.

The next is the story about the hermit Ero, or Hero, told by Palladius, bishop of Helenopolis. I have never seen the original, and the versions given of it are at variance. According to Astruc² this worthy, who was a glutton and a profligate, caught from an actress a "Carbuncle in the Glans," the result of which was that within a fortnight (corrected farther on³ to six months) the whole of the genitals mortified and "dropt off," after which the patient became marvellously devout. The other statement I take from Haeser, who says⁴ that the disease took six months to effect this destruction, and ended in the death of this exemplary personage. Astruc's mistake in translating anthrax by "carbuncle" is easily corrected by Haeser's version, which renders it by "mortification." But I do not understand syphilis or any other disease destroying the organs, or, to use the coarse phrase in the text, "his private Parts." If this expression be interpreted to mean the penis, I see no objection to it, providing the accuracy of the first date given by Astruc be verified, as this tallies with the accounts in later writers. For instance, Pearson speaks of sphace-

¹ *Nouveau Traité des Maladies Veneriennes*; 1861, p. 4.

² *A Treatise of Venereal Diseases*. By John Astruc. London, 1754, p. 15.

³ *Ibid.*, p. 58.

⁴ *Lehrbuch der Geschichte der Medicin*; 1865, B. 2, S. 193.

lation of the whole penis taking place in less than a fortnight, possibly the very form of primary sore for which Avicenna is said to have suggested amputation of the penis, and which was so common in the middle ages ; whereas I know of no disease which takes six months to do this mischief. Mr. Lee however mentions¹ a case of slow phagedænic ulceration by which the whole penis was gradually lost, and another where part of the member was thus destroyed.

But though scattered notices of this kind are to be found, more than half the time that has elapsed since the days of Celsus was to roll away before light fairly breaks upon the scene ; for it is not till we come to the writers of the thirteenth, fourteenth, and fifteenth centuries, that we meet with evidence to which we can appeal as decisive in support of the position, that primary disease, at any rate, was then known. When, however, the proof does reach us, it is in a form strong enough to convince even those who are indifferent or prejudiced. In various great seats of learning and research, Milan, Bologna, Montpellier, Oxford, we find, from about the year 1270 onwards, a somewhat frequently recurring, and even more precise mention of facts, showing that the attention of thinking men had been drawn to the occurrence of these lesions ; and the notice of them, at so many different places and by so many different authors, seems to me decisive proof of their being already widely diffused through western Europe, while of their identity with many of the sores in the present day I fancy there can be little doubt. For nearly two centuries and a quarter before the great outbreak of syphilis, we may notice reiterated mention of such symptoms as bubo, "imposthume of the groin," "ulcers of the yard," "ulcers from pustules of the yard," "mortification of the substance of the yard," all attributed to "lying with a foul woman," "a foul, nasty or cancrus woman," a "woman having an ulcer of the womb," and so on. We may read a description in at least one author, of sores piercing between the skin and flesh of the virile member, the very feature which Judd² gives as characteristic of the "black lion" "burrowing under the foreskin

¹ '*Lectures on Syphilis* ;' 1875, p. 148.

² '*A Practical Treatise on Urethritis and Syphilis* ; 1836, p. 197.

and dissecting between the integument and the body of the penis."

The credit of having honestly and lucidly attempted to show that syphilis is not the modern disease it was long supposed to be, must I believe be apportioned to Beckett,¹ one of those bold and original thinkers who leave the impress of their minds on the task they take in hand. After consulting "a great Number of ancient Physical and Chyrurgical Books," he concluded for the antiquity of the disease, and he laid the foundation for the change of opinion which he wished to effect; for though he weakened his argument by incorporating, after the theory of his day, gonorrhœa, the "Sycknesse of Brenning," with syphilis, and deciding rather hastily leprosy to be the latter disease, and though he was afterwards overshadowed by the monstrous erudition and ponderous arguments of Astruc, yet his time came, and perhaps he, more than any other man, contributed to establish the antiquity of at least two of the diseases then known as syphilis. Now Beckett tells us that John of Gaunt died in 1399 of phagedæna of the genital organs, and that, on Richard the Second paying him a visit, "time-honoured Lancaster," who by the way seems to have been a person of decidedly loose morals, "magnus enim fornicator fuit" being the character given him, favoured his majesty with a display of the ravages which the disease had made. Such at least is the story as given by Master Thomas Gascoyne, then Chancellor of Oxford, who also mentions that several persons of distinction in that time suffered in the same way. To this may be added the statement of Simon² that in 1414 King Ladislaus died at Naples of phagedæna.

True phagedæna rarely if ever kills, and therefore the disease spoken of, supposing it to have been really the direct cause of death, was most probably sloughing sore. The universal debauchery, which prevailed during the middle ages in some parts of western Europe, has been suggested as the reason why this phagedæna, or sloughing, was so frequent; but it is doubtful

¹ *An attempt to prove the Antiquity of the Venereal Disease long before the Discovery of the West Indies, &c.* By William Beckett, Surgeon. *Philosophical Transactions*; 1718, p. 839.

² *Ricord's Lehre von der Syphilis*; 1851, S. 7.

whether such dissoluteness was the rule in England at that time, though the habits of both sexes were uncleanly and unhealthy enough, and though drunkenness and gluttony were to be seen on every side. Haeser thinks¹ it feasible that phagedæna (sloughing) was prevalent in antiquity, and that this interfered with the development of secondary disease, which may also have been the case in the fourteenth and fifteenth centuries.

What then does all this mean except that the complaints so described were primary sores? Waiving the question of unity or duality, it may be safely asked, to what other disease than syphilis are we to ascribe hard, almost dry, sores, phagedæna, and buboes? If we can here and there explain away an individual symptom, can we honestly adopt this process with regard to the aggregate? For my own part I know of no malady to which such symptoms can, on any plausible grounds, be referred, except the one which has always given rise to them, namely syphilis itself. It is certain that, in the thirteenth, fourteenth and early part of the fifteenth century, when men were insensibly paving the way for such momentous struggles with the pedantry of the schools, and had begun to think for themselves, several authors reasoned correctly enough as to the origin of these sores from what is after all the great source of syphilitic contagion, namely connexion with unclean persons; and I need scarcely say that a patient, who would nowadays plead an innocent origin for the sores described by Bernard Gordon or John Gaddesden, the "*puncturæ inter carnem et corium*," would find his story received with great want of faith.

I consider then, as just pointed out, the wide-spread existence of such sores as irrefragable proof of the prevalence of local syphilis itself, for, in direct opposition to the opinions of some eminent authorities, I look upon ulceration of the genital organs, *from any cause but suspicious intercourse*, as very rare. Richard Carmichael says² "the very organization, secretions and functions of the genitals dispose them to ulceration, beyond all other parts of the body." My experience is the reverse, and I consider it strange that, if Carmichael be right, I should, in so many cases of

¹ *Op. citat.*, Vol. 2, p. 186.

² *An Essay on Venereal Diseases*; 1825, p. 19.

venereal and supposed venereal disease, about which I have been consulted, and in the great number of patients suffering from diseases of the skin treated by myself at St. John's Hospital, have very rarely met with spontaneous ulceration of the genitals, though constantly on the watch for such morbid appearances. Herpes, eczema and lepra often assail the penis and scrotum, and may end in superficial excoriation ; scabies, too, will, as is generally known, set up pretty large pustules on the penis, but I know of nothing which supports Carmichael's assertion.

We are then, I contend, warranted in looking upon it as proven, that, long before the outbreak of syphilis in 1494, *primary symptoms existed which can be traced back to no other disease than the venereal* ; for it may save some confusion if I say at once, that I do not, and never did, consider secondary symptoms as the exclusive fruit of the hard sore, using the term in its strict sense. On the contrary, I have reason to think that infecting sore, in some rare instances, while yet quite distinct from chancroid, does not pass through the phase of hardness ; and that on the other hand in some instances, also very rare, certain slight and obscure symptoms, of slow and incomplete evolution, follow sores, the history and look of which do not in any satisfactory way distinguish them from chancroid, which is yet, however, so far as such evidence can be supposed to carry weight, to be looked upon as a self-existent variety of syphilis. Again it is certain that *in our day, and ever since syphilis was first understood*, some of the primary symptoms, mentioned by the authors I have previously spoken of, *have always been and still are, in a certain proportion of cases, followed by secondary disease*. Adams reports¹ a case of secondary and tertiary affections succeeding exactly the phagedæna described by Celsus, and he who has seen much of syphilis has seen the same thing.

Absence of constitutional Syphilis.—But there is no proof of systemic infection having, in these early times, ever followed such lesions ; everything goes to show, either that the sores of the two forms of syphilis, alike in the era of Celsus, or Gordon or Hunter, sometimes approach each other so closely as to baffle and perplex the best observers ; or that sores, now followed by

¹ *Op. citat.*, p. 32.

constitutional disease, did not, at one period of historic medicine, entail this penalty. Till we come to the incomplete case mentioned in the consultations of Hugo Bencius, we find no description which can really pass for a faithful portrait of lues. Utter silence about constitutional syphilis taken as a whole, absence of all reliable account of it taken as a sequence and group of symptoms, meet us, turn where we will. We hear indeed, long before the invasion of Italy by the French, of two diseases which some authors have unhesitatingly pronounced to be syphilis, but which I feel compelled to reject with equal want of hesitation.

The first of these is the form of leprosy said to have been communicated by contact. The opinion that this was constitutional syphilis, unrecognized and mistaken, has been supported by an array of historical testimony for which I cannot find space, but as the question is one of considerable importance, it can be better taken when that of the recognition of syphilis as the leprosy of the ancient writers comes under review. For the present it will, I think, suffice to say, that all the most reliable evidence is in favour of the view, that the disease thus confounded with syphilis was simply elephantiasis Græcorum, and that the idea of its being contagious rested on a mistake. My own experience, certainly, quite supports the theory of non-contagion; and I do not see how any one can duly weigh such testimony as that detailed by Bateman,¹ that in the report of the London College of Physicians on leprosy, and the facts related by Dr. Gavin Milroy,² without coming to the conclusion, that this disease is rarely if ever transmitted even by so potent a means of communication as sexual intercourse. Yet in the face of all this evidence we are told,³ that the belief in the contagious nature of leprosy, so strongly implanted in the minds of the laity, is shared by some members of the medical profession. An error, then, prevalent in our day, is conceivable enough in times so far removed.

The other disease was the malmorto, an affection which in some of its features resembled constitutional syphilis as much as

¹ *Practical Synopsis of Cutaneous Diseases*; 1819, p. 304.

² *Report on Leprosy and Yaws in the West Indies*; 1873.

³ *Pathology and Treatment of Diseases of the Skin*. By J. L. Milton; 1872, p. 288.

leprosy does, and which was frequently accompanied by buboes. According to Simon¹ one symptom was a growth of rupial crusts, the description of which corresponds with that of the eruptions seen in persons labouring under syphilis, after the retreat of the French from Italy; and which de Vigo, who remarks² upon the close resemblance of this disease to the syphilis of this day, having the same signs and causes as the “frenche pockes,” describes³ as “a maligne, filthy, and corrupte scabbe,” breaking out on the arms, legs and thighs, the complaint being acquired by contagion sometimes derived from a leprous woman, and when confirmed only admitting of a “cure palliative.” This disease was cured, we are told,⁴ with mercury pushed to the extent of salivation; a practice adopted, not merely by rude charlatans, but by some of the most eminent physicians in the middle ages, such as Theodoric and Arnald of Villanova. What malmorto really was I confess my entire inability to decide. It may have been, as some authors have thought, inveterate scabies, but I think there can be little if any doubt that it was neither true leprosy, which is not curable by the use of any preparation of mercury, nor syphilis, for there is no account anywhere of the antecedent symptoms proper to this disease.

Again it may be observed, without referring the reader here on every occasion to authors and dates, that a good deal of scattered evidence has been collected, showing that at one time or other, ages before the great outbreak of syphilis, people suffered from affections which we should look at very suspiciously if met with among young men of the present day. Among these are condylomata, fungous growths on the genitals connected with disease of these parts, rhagades, ulcerations of the mouth, destructive ulcerations of the velum and palate, as also of the nose, and nodes known in the old english tongue as boonhaw,⁵ swelling of the bone, not improbably the vitium of Celsus.⁶ J

¹ *Op. citat.*, p. 9.

² *The most excellent Workes of Chirurgerye . . .* by maister John Vigon; 1543, Fol. clxiii.

³ *Ibid.*

⁴ Simon; *Op. citat.*, p. 9.

⁵ Beckett; *Philosophical Transactions*; 1720, p. 52.

⁶ Adams; *Op. citat.*, p. 40.

suppose it is quite superfluous to say of each of these symptoms, that we might safely refer it to syphilis, *provided there was a history of syphilitic affection to connect it with*, or some other, even solitary, sign of venereal taint, but this is precisely what is always wanting. At one time I felt inclined to refer all such tokens to venereal disease, but the more I searched into the matter, the more satisfied did I become, that the basis of a strict induction was in every case faulty; and that some of the symptoms mentioned, such as condylomata, might with equal confidence be ascribed to the transformation of soft primary sore, and others, *e.g.*, the affections of the nose, mouth and velum, might be due to leprosy, while of the nodes I can offer no explanation.

It seems a lame conclusion to arrive at, when an author is driven to admit that he must leave the case exactly as he found it, but I see no alternative except to do so; and when the materials for thoroughly solving a question are wanting, the cause of true knowledge is best served by confessing our inability to decide it. Here we see, arrayed on one hand, primary symptoms, some of which are now in a certain per-centage of cases inevitably succeeded by constitutional disease, existing for ages without our being able in a single instance to follow them up to their natural results; and on the other hand, constitutional symptoms which one might think could scarcely have always been of innocent origin, which we are equally unable to trace back to a primary lesion. Such being the case, I feel that I ought to remit to other hands the solution of an enigma which quite puzzles me.

First Traces of true Syphilis.—As we approach the era of the so-called siege of Naples, we come upon evidence of a more precise kind, and which, however faint and dim may be the first feeble impress it stamps upon pathology, gradually acquires, with the lapse of time, a force and completeness which, in my opinion, leave us no alternative but to admit, that *the earliest appearance of true syphilis in Europe must henceforth be materially ante-dated*. The famous traveller, Dr. Clarke, in his account¹ of a manuscript in the library at Stockholm, by Johannes Arderum de Sleewarck, which must have been written rather early in the fifteenth century, inasmuch as it is the work of a man who practised at Neewarck

¹ *Travels.* By E. D. Clarke; 1824, Vol. xi, p. 139.

from 1412 to 1419, says "It is very curious to observe (in this manuscript), 'Pro morbo qui dicitur . . . ,' followed by the French name of a disease, which is supposed not to have been known before in Europe before the discovery of America." Very curious indeed, for if this be not a later interpolation, there is an end to all doubt about the origin of syphilis having taken place much earlier than is generally supposed; it being, I imagine, certain that morbus gallicus or some equivalent is meant here, and that such a term was at the outset only applied to true syphilis.

But there are grounds for believing that it is not a later interpolation. Haeser gives evidence in support of the theory that morbus gallicus is derived, not from the classic name for France, but from the vulgar word galle (gâle, itch, a disease which has on numerous occasions been mixed up with syphilis), the morbus gallicus having been at one time in France called galle (gâle), used in England as far back as the fourteenth century to designate a running sore. He adds¹ that the term Mala Franzos was known in Germany as early as 1472, an official record showing that in that year a choir singer of Mayence was relieved from his duties because he was suffering from that complaint. Dr. Hermann Friedberg rejects² Haeser's conclusions, on the ground that the german authors called the disease gallicus, not for any such reason, but distinctly because they thought it came from the French; but though this gentleman has treated the subject in a masterly way, I still think he fails to refute Haeser's opinion. Of one thing however we may feel pretty well assured, which is that the old myth about the name dating from the french invasion of Italy can be given up.

The next piece of evidence that we come to is the consultation of Hugo Bencius, who died in 1448, and which may therefore be even earlier than the notice of venereal disease mentioned by Clarke. It is taken from Astruc,³ who treats with derision the idea that the disease could have been syphilis, in which Haeser seems to concur, and it must be admitted that the picture

¹ *Op. citat.*, Vol. 2, p. 223.

² *Virchow's Archiv.*, B. 33, S. 291.

³ *Op. citat.*, p. 46.

is not so clear as one could wish. Astruc gives a long list of symptoms, the diagnosis and classification of which might puzzle the whole faculty and stands open to accept any verdict but that of their being syphilis. Among the symptoms we find pain in the head lasting "a Month and a Half;" pimples about the scapula, the size of peas or filberts; "a hard Tumour on the back Part of his Legs;" feverish fits; eruption of red spots, somewhat rough, from neck to hips; night pains; tumour on right leg which ulcerated and was followed by red, rough, scaly spots and boils on different parts of the body. I suppose the proper decision to come to here is that we cannot say with certainty what the disease was, but that some of the symptoms remind one very strongly of syphilitic pains, lepra, gum knot and possibly impetigo rodens. As concerns merely my own diagnosis, I should be disposed to say at once, that I know of no disease except syphilis to which such symptoms can be ascribed.

Again, as Friedberg points out,¹ a pestilence is spoken of in the *Annales Danicæ* of Peter Olaus for 1483, as raging in Denmark, and is called the morbus gallicus. Pinctor says the disease began in this year. Hensler, indeed, translates with some hesitation the passage which he quotes to this effect, but I do not see how it can be rendered otherwise.² But as the reader will see, Pinctor arrives at this conclusion, not on any historical data, but because he was one of those, out of whom fire could not melt the conviction that syphilis owed its origin to sidereal changes, and these took place in 1483. M. Auspitz makes him say that it began at Rome in that year, but the word Rome does not, so far as I can find out, occur in Pinctor's account of the origin of this disease, and he did not go there till ten years after, so that he might, inasmuch as the text is concerned, be referring to its beginnings in Valencia, to which view Hensler clearly inclines.³ The real difficulty, however, with regard not only to Pinctor but

¹ *Virchow's Archiv.*, B. 33, S. 287.

² "Hic morbus cepit exordium anno 1483, quid eo a. de M. Octobr. fuerunt 4 planetæ, videlicet Jupiter, Mars, Sol et Mercurius in libra in domo ægritudinis ipsum morbum durasse per aunos xxvii (xvii) numerando a principio morbi scil. ab. a. 1483."

³ *Ueber den westindischen Ursprung der Lustseuche*; 1789, S. 92; *Geschichte der Lustseuche*; 1783, S. 112.

to many writers of that day, is that we cannot always feel sure whether they refer to syphilis or some of the numerous epidemics which then prevailed.

Peter Martyr, as is well known, in a letter to the great portuguese scholar, Arias Barbosa, dated April 5th, 1488, the year given by Delgado for the appearance of syphilis at Rapallo near Genoa, mentions the disease as existing in Spain, in the most unequivocal terms, and calls it morbus gallicus. The correctness of the date has been disputed, partly on the ground that the letter was addressed to the greek professor at Salamanca at a time when no such professorship was in existence, by Thiené,¹ who however wrote in an age when men believed as firmly in Astruc's sophistical reasoning as they did in the circulation of the blood; but so far as I can judge Mr. Prescott has quite disposed² of the objection. Haeser considers the date to be of undoubted accuracy, while Friedberg taxes Thiené with not having understood the Latin which he relies on, and having translated as "university professor" what might clearly apply to any teacher. Lastly we are informed, that some of those who saw the disease in 1494 and 1495 were already acquainted with it. Thus Hensler quotes,³ and with very natural surprize, the account of Schellig (1494 or 1495), which does not contain a word about the disease being new, but quite the contrary; whilst Schellig's editor, Wimpfeling, (1494-95) distinctly says it was not new. Farther on⁴ Hensler cites from Salicetus a passage showing that syphilis existed as early as 1457, which date he thinks might very well mean 1475, because at that time men frequently wrote the numbers as they spoke them. Of the authors who speak of this disease having appeared so early as 1492 and 1493 at Rome and elsewhere, it will not be necessary to give any particular account, as their dates are entirely forestalled by those already mentioned.

On the other hand Alexander Benedict, a venetian physician (1495), Leonicens, "Professor of Physic" (1497), Conrad

¹ *History of Ferdinand and Isabella.* By William H. Prescott; 1851, Vol. 2, p. 202.

² *Ibid.*, p. 203.

³ *Geschichte der Lustseuche*; S. 9.

⁴ *Ibid.*, p. 13.

Gilinus, "Doctor of Arts and Physic" (1497), Bartholomew Montagnana the younger, "Professor of Physic" (1497), Caspar Torella, physician (1497), Wendelin Hock, "Professor of Arts and Physic" (1502), and Anthony Benivenius (1507), are quoted by Astruc as considering the disease to be new.¹ Hensler flatly denies this of Leonicens, and the proofs he cites² seem quite convincing. Taking, then, those who remain, we must pronounce their testimony inadequate to overthrow that on the other side. No doubt the disease was new to them, but this only shows that it had not reached that part of the world where they were residing. Syphilis is spoken of by Martyr, Delgado and Fulgosi as appearing in Spain in 1488, Rapallo 1488, and Rome 1493. Of the six authors quoted by Astruc two if not three, Benedict, Montagnana, Hock, were in Venice and Padua, and one, Beniveni, in Florence, parts only invaded apparently much later by syphilis; one, Torella, was a wanderer who wrote his account at Blois, and spoke of syphilis as beginning in Auvergne in 1493, and being in reality known to the ancients;³ and of the other, Gilinus, nothing is known, but as he dedicated his work to the duke of Esté, he too most probably lived at some distance from the first foci of the disorder. The ignorance of these authors about the earlier appearance of the disease expressly refers to its outbreak in 1495, and in this they were manifestly at fault. The evidence that it broke out sooner is, taken as a whole, too strong for any rebutting testimony. Exception might be made to parts, but looking to the aggregate, I consider we may pronounce the existence of syphilis in Spain and Italy long before 1494, to be as well established as any fact in history, and Hensler says⁴ the evidence of syphilis being prevalent in Upper Italy and Rome in 1492 and 1493 is incontestable. At the same time I think such evidence as that adduced from Salicetus is to be received with great caution. Obviously enough he may have been right, or memory at so long an interval may have played him false; we have no means now of deciding such a point. It is a question of veracity and ac-

¹ *Op. citat.*, p. 32. I have corrected the dates from Hensler.

² *Geschichte der Lustseuche*; S. 43.

³ *Ibid.*, S. 35.

⁴ *Ueber den westindischen Ursprung der Lustseuche*; S. 16.

curate observation ; one of those matters in which we implicitly trust one man and hear another in silent doubt, and therefore till much stronger proof is brought forward I should vote for rejecting the story.

Vella saw the disease both before and after the outbreak of the morbus gallicus, and expresses his surprize at finding it, from that date, followed by secondary symptoms, the initial lesions appearing to him identical. M. Bassereau maintains¹ that this is an error, and that the exact similarity spoken of by Vella never existed, the best physicians of that day having pointedly noticed the hardness and different tint in the new form of chancre. This objection is a little tinctured with extreme dualism. No doubt Vella was wrong ; he might have discriminated better between typical hard infecting sore and chancroid ; but he was only wrong in this much, that he converted an occasional resemblance into a rule. Everyone, familiar with syphilis, knows that the first of these lesions will almost inevitably be followed by secondary disease ; but he knows quite as well that cases constantly meet us, where we cannot say, either from the look of the sore or the results of inoculation, whether this consequence will follow or not. Some of the best surgeons, including M. Ricord himself, have not shrunk from confessing that they could not always decide upon the nature of a sore from its visible signs. It is therefore easy to understand how Vella fell into such a mistake, and indeed it would be rather surprizing if any person in his day, who saw much of syphilis, had always avoided an error which is now by no means infrequent. I hope however to show that Vella unconsciously helps, not only to establish a most important truth, *the duality of syphilis as a historical fact*, but to explain the confusion about primary sores which has arisen from confounding similarity with identity.

Syphilis recognized as the Leprosy of the Ancients.—We are told² that some of the physicians, who at the close of the fifteenth century saw syphilis for the first time, so far from looking upon it as a malady hitherto unknown, thought it was a return of the ancient leprosy, with the symptoms of which, those of them who

¹ *Traité des Affections de la Peau* ; 1852, p. 239.

² Simon ; *Op. citat.*, p. 4.

had read the works of the arabian writers were quite familiar. The author, from whom I quote, adds, that the names under which they recognized this leprosy were bothor, saphati and formica, which were clearly thought to be individual maladies, forms of leprosy ; but the fact is that there never were such diseases. They belong to the “inane Spectralities and Cinder-heaps, presided over by Dryasdust and Human Stupor ;” and those who endeavoured to trace syphilis back to such turbid sources, attempted a task impossible of execution, and of which they were perpetually losing the clue.

Bothor is defined by Kraus as meaning little bladders and pustules, such for instance as aphthæ in the mouth, acne spots and small-pox pustules, a meaning which is at any rate comprehensive enough ; according to some authors it includes eczema. Of this strange malady saphati was, according to de Vigo, a species. Saphati itself, following the account given by Fournier,¹ was a papulo-squamous affection, as much like syphilis as anything else, but very difficult if not impossible to identify ; indeed his researches satisfied this careful observer, that most probably several different affections were confounded together under this name ;² while Hensler, who enjoyed extraordinary facilities for examining old authors, considers³ saphati to be the same thing as the purulent scalled head of his day, which was most likely inveterate eczema or some form of tinea, complaints having no analogy whatever with any papulo-squamous eruption. Hensler's definition is still farther expanded, and the confusion thereby increased, in his work on leprosy. From the picture drawn of it by John de Vigo, this disorder appears to have been more like acne rosacea, which disease however was also known to him. “Saphati,” he says,⁴ “are little pustles whiche are engendred in the foreheed, in the neck and in the face, and cheefly about the nose, and are fleshye with a lytle crust upon them lyke the skale of a fysh,” which “appeareth also in the Frenche pockes.” System, standing on the undisputed basis of the four humours, required that there should be four kinds of saphati and four were found, the third in the series being

¹ *Jean de Vigo*. Par le docteur Alfred Fournier ; 1870, p. 80.

² *Ibid.*, p. 128.

³ *Geschichte der Lustseuche* ; S. 21.

⁴ *Workes of Chirurgerye* ; Fol. cxxxi.

an eruption of papules of a very peculiar nature, for they were not accompanied by either itching, inflammatory redness or moisture.¹

Lastly formica appears to have been more akin to lupus than to anything with which I am acquainted, a fact possessing a certain degree of interest when connected with Swediaur's account of syphilis in Canada. De Vigo however describes² formica as a "lytle pustle or many pustles that come upon the skynne," and tacitly assents to Avicenna's view, that "every Aposteme walkynge in the skynne, not having brodeness is a Formica." In other words both these writers considered it to be an illness which corresponds to our impetigo. To these three complicated disorders Haeser, who discusses the question carefully, adds³ aluhumata and thymius of which I will not trouble the reader with any account. It appears unnecessary to multiply this kind of evidence. Carrying it farther would simply add to the bulk without increasing the value, and I certainly think enough has been said to show the utterly unreliable nature of such recognitions.

The cause of much of these erroneous views lay in the idea which prevailed about the excessively complicated nature of leprosy itself; for the literature of this disease was, at the close of the fifteenth century, in a state of the most hopeless confusion, which had begun with the first mention of the disease by those, to whom for so many ages men had turned for information, and had gained strength and extent with years. As Dr. Adams puts the case, the Greeks knew nothing of the arabian leprosy, spargosis; the Latins were equally ignorant of this and (knew almost as little of) leprosy; while the Arabians called spargosis by the name of elephantiasis, and elephantiasis græcorum by that of leprosy. Nor could the latter even agree among themselves as to names; for while Rhazes, the oldest of these writers except Serapion, calls leprosy, lepra, Haly Abbas, according to Hillary,⁴ terms it elephantia, and Avicenna gives it both these names. To make the state of matters still more bewildering, we are told, what we can very well believe, that the account was farther complicated by the

¹ Hensler; *Vom abendlaendischen Aussatze*; S. 74.

² *Workes of Chirurgerye*; Fol. xxi. ³ *Op. citat.*, Vol. 2, p. 242.

⁴ *Observations on the Changes in the Air, &c.*; 1759, p. 323.

errors of transcribers. Round this promising nucleus of perplexity there had gradually gathered a cumulus of theories, systems, and definitions ; every author having for ages added something to the confusion, till at last leprosy, complicated even in the days of Moses, had become a perfect nightmare, and such it remained till Adams separated lepra from it, and Bateman, confessedly unable to see his way out of the maze in which the two forms of elephantiasis were entangled, called in the aid of Mason Good, whose wonderful learning and acumen enabled him to separate, for the first time, the compound into its component parts.¹

In the works of the earlier greek and latin writers, and in those of the arabian and later greek writers, we can recognize several of the skin diseases now seen daily in our hospitals, and an almost unbroken line of medical writers had continued the knowledge of these down to the end of the fifteenth century, re-describing them often with a considerable degree of fulness and accuracy. Some, it is true are not very clearly defined, but yet so far separated from all other cutaneous affections that we may feel pretty certain of their individuality. Of these no less than sixteen—twelve belonging to the clearly recognized, and four about which we can only say that there is no other disease to refer them to—were reckoned as species of leprosy, produced by the leprosy virus, and capable of being converted into this disease. These affections were first of all leprosy itself, early distinguished as tubercular leprosy ; lepra (psoriasis) and leucoderma, which are so constantly called leprosy that it is often not easy to make out which disease some particular author is speaking of ; and elephantiasis, which is treated as being indisputably a local form of tubercular leprosy, the poison having shown itself upon the leg or the scrotum instead of diffusing itself through the whole frame. After these come white and red pityriasis, about the leprous nature of which no doubt was entertained ; alopecia and sycosis, almost equally well established ; impetigo and erythema ; kerion and tinea versicolor, not so widely recognized, but still never rejected so far as I am aware. These are followed by the four less certain forms, namely vitiligo (morphea or morphea alba) acne rosacea, tinea circinata which may however have been

¹ *The Study of Medicine* ; 1829, Vol. 5, p. 590.

ring-lepra, and scabies which seems to have been "the itching leprosy." Again these components themselves were often, apparently for no other reason than because they were supposed to constitute so many divisions and subdivisions of leprosy, arbitrarily ranked with other diseases in a most embarrassing way. Thus lepra was classed with leucoderma to form a genus, which was divided into species, while the group of affections constituting morphea nigra seems to have included inveterate ichthyosis. Amidst all this confusion tubercular leprosy seems to have always been looked upon as the type and parent disease, and when Beniveni and de Vigo speak of leprosy being no longer known, it is clearly to this form that they refer.

It is therefore not at all surprizing that the medical men, at the close of the fifteenth century, took up rather a misty idea of such a multiform complaint, and that the greatest conflict of opinion prevailed among those, who thought they recognized in syphilis the leprosy which had scourged so many nations and so many ages. Sebastian of Aquila considered the new disease to be leprosy; Widman ranked it with saphati, with this addition however that it at the outset resembled formica, an exactly similar opinion being, as I understand Hensler, held by Montesauro, while Leonicens stoutly maintained that syphilis had never before been described as any form of leprosy, an opinion also upheld by Cataneus and others¹, and in which I concur so far as the idea of anything like a full description is concerned.

If it be asked how it came to pass, that men ever traced syphilis back to a monstrosity like the picture of leprosy, which had been expanded till it embraced the fundamental lesions of at least five-sixths of all cases of skin diseases, why they failed to see how unreal the old account was, I reply that most likely several causes operated. The writers of that day were fond of tracing affinities between diseases, and sometimes carried this to such a pitch as to connect maladies, which required an immense amount of faith and a vigorous effort of the imagination to bring them into relationship. Thus de Vigo went so far as to fancy that he had discovered in syphilis the malady under which the emperor Augustus suffered. This has been spoken of as, and no

¹ Hensler; *Vom abendlaendischen Aussatze*; S. 229.

doubt is, an extraordinary mistake for so able an observer, seeing that there is no resemblance between the two affections ; but it is quite matched by the discovery that *tinea decalvans* and *tinea versicolor* are forms of leprosy, as was maintained by a writer of great eminence at the close of last century.

But respect for authority evidently played the chief part ; of this I apprehend there can be little doubt. The men, with whom Leonicens had to contend, feared to trust the evidence of their own senses, and to assume that they had met with a disease unknown to Hippocrates and Galen, to Rhazes and Avicenna. Even the pathology of a disease was incomplete unless it could be divided into four species corresponding to the four humours of Galen, and remained so long after Paracelsus openly burned his works at Basle, foretelling that the author and Avicenna would one day be served after the same fashion.¹ No doubt this was ridiculous subservience on their part, but such deference to authority continued through ages which claimed to be enlightened, and is anything but extinct at the present day. We find Bateman taxing² Dr. Hillary, who had seen leprosy in Barbadoes, with describing the complaint, not as he found it more than two centuries and a half after Charles the Eighth's entrance into Naples, but as he had read of it in Aretæus, and indeed some of Hillary's statements, such as those about the curability of leprosy in its early stages, are far more in accord with tradition than fact ; while some of the most important features in the pathology of the fifteenth century are still religiously preserved and taught in our classics of medicine. Our present division of temperaments, with their visionary bias to certain diseases and to be influenced by certain remedies, our scrupulous dread of repulsion of disease, and the tendency to refer all inveteracy to scrofula, gout or syphilis, are but far off reflections of what was so long taught about the galenic humours and leprosy. The theory, so clearly propounded by that excellent observer, Cataneus, about the mischief done by cauterizing syphilitic sores and thus driving the poison into the system, is hardly cold in its grave.

It seems strange that those, who were seeking for the prototype

¹ "Sic vos ardebitis in Gehennâ."

² *Op. citat.*, p. 303.

of syphilis in the leprosy of the dark ages, never thought of comparing the new disease with leprosy itself, and the explanation offered of their neglecting so practical a test is more startling than the fact itself; for we are told that it was because the leper had disappeared, had become a thing of history. I have already mentioned that Beniveni and de Vigo speak of true tubercular leprosy as a disease almost entirely unknown at that time, and all the evidence to the contrary is slight and of uncertain value. Astruc, an author whom I always cite with reluctance, says¹ that the lepers refused to associate with the venereal, but the author he quotes from wrote forty years after the outbreak of syphilis, and the lepers of the time he speaks of were chiefly sturdy beggars, with itch or some other skin disease, on the strength of which they claimed the charity of the leper house. But it is going too far to assert, as a modern author has done,² that there were no lepers at all. Beniveni saw a case, and only says that it was very rarely seen in Italy. It still lingered in some parts of Germany, though very occasionally met with; and it either prevailed in districts quite near to the principal arenas in which syphilis first showed itself, that is to say certain maritime and rural districts in Italy, Spain and France, or it only passed away from these parts to return and infest them again, as the evidence of leprosy existing there at a later date seems quite reliable.

And though it was fading away from cities and towns, though its haunts might be out of the way of the followers of medicine, men could not have forgotten what leprosy was. If the leper were not to be seen in the streets, if the priest no longer read the funeral service over him while living, and the law no longer disinherited him and severed him from the rest of the human race, he was yet too near the time for the memory of his disease to have passed away, and indeed Hensler professedly³ drew his materials from those writers who, between the twelfth and the end of the fifteenth century, had themselves seen and described the disease. Besides at that time even popular opinion was hardly likely to be

¹ *Op. citat.*, p. 10.

² "Il est très-clair que les medécins de cette époque n'avaient point vu de lépreux;" *Traité théorique et pratique de la Syphilis*. Par Armand Després; 1873, p. 72.

³ *Vom abendtaendischen Aussatze*; S. 2.

much at fault, so long as ever a case was here and there to be found. The rudest people have always been able to recognize genuine leprosy, the dreaded and detested first-born son of death ; ignorant of refinements in diagnosis, they seize at once upon those features which have made it a subject of horror in every age and to every race of men. They might confound other diseases with it, but I should think that leprosy, *differently from syphilis*, was never yet taken for anything else. A man, who has read a description of it years before, will recognize it at the first glance, and once seen it is never forgotten. For such reasons I must conclude that tubercular leprosy, which does not seem to have changed in a single feature or quality since it was described, was too well known to the physicians of Charles the Eighth's day to permit of their recognizing syphilis in it. Such a mistake could only have been made by a very inexperienced writer, who had neither seen nor heard much, though it is conceivable enough of the concourse of symptoms which made up the classic picture of leprosy.

To those who urge that it is now mere waste of time to confute such errors, that they have died out or will die out of themselves, I reply that it is not so ; that they cannot be trusted to execute the happy despatch, and that we must perform that sad office for them. So recently as 1863, an author, who has not I believe withdrawn a word of what he then said, and whose name secures an enthusiastic reception for any opinion he may choose to express, M. Ricord, remarking upon the possible connexion between leprosy and syphilis, stated his belief in the existence of an ancient leprosy distinct from that known to us. In order that I may not be supposed to have in any way misrepresented his meaning, I give M. Ricord's exact words : " La lèpre des Grecs ou des Arabes que nous connaissons aujourd'hui, est elle semblable à cette lèpre antique ? Nullement, car la lèpre d'alors était souvent contagieuse, elle se communiquait par les rapports sexuels. Evidemment ce n'est par notre lèpre actuelle."¹ No person can complain here of any want of precision, either in the language used or in the opinion laid down ; but indeed even those who differ most widely from M. Ricord, must admit that he

¹ *Lettres sur la Syphilis* ; 1863, p. 157.

always speaks his mind with a clearness and thoroughness which are highly to his credit.

I have the misfortune to differ very widely from him on this subject, and must regret that M. Ricord does not refer us to at least one author for the description of a leprosy quite distinct from that of our day, seeing that I have entirely failed to meet with any such thing, and that I must, judging from the evidence already laid before the reader, believe that a contagious leprosy is in its way quite as unnatural a combination as a harpy or a dragon would be. That many of the old authors distinctly say leprosy is contagious and hereditary, just as we now speak of such properties in syphilis, may at once be admitted. The description given by Bartholomew Glanville might pass for an ancient portrait of syphilis itself. This author, who according to Hensler was a nobleman of the house of Suffolk, and who flourished about the year 1360, in his book *De Proprietatibus Rerum*, a work which contains a faithful reflection of the most reliable opinions about leprosy, and particularly those of Constantine, and which was translated by John Trevisa, vicar of Barkley,¹ tells us that the complaint “comyth of fleshly lyking (Qy. lyging) by a Woman after that a Leprous Man hathe laye by her” (a statement which the reader is invited to compare with the caution given by Widman² soon after syphilis began to have a literature of its own); “also it comyth of Fader and Moder; ann so thys Contagyon passyth into the Chylde, as it ware by Lawe of Herytage. And also when a Chylde is fedde wyth corrupte Mylke of a Leprous Nouryce.”

But Glanville, like every author of repute who describes leprosy so that we can identify it at all, portrays a malady which corresponds to that of our age in every respect except that of its being contagious, a quality which I think we may safely refuse it. And if those who take M. Ricord's side of the question demand why we are to distrust on one point those whom we trust on another; why we reject a long-established principle laid down by

¹ Becket. *Philosophical Transactions*; 1720, p. 59.

² “Summopere tamen cavendum ne coïtus fiat cum muliere pustulatâ, imo neque cum sanâ, cum quâ prius brevi temporis spatio concubuit vir pustulatus.”

a succession of men evidently quite masters of the symptoms and course of the disease, I reply "by reason of the law which has ever governed the selection of evidence." When we find the symptoms of a disease unchanged at the lapse of several hundred years, as is the case with respect to leprosy, we may confidently assume that the earliest observers were in the right; when on the other hand we find that a contagious property, the belief in which is generally firm in proportion to men's ignorance of the case, becomes more and more dubious as leprosy is more carefully investigated, we are justified in doubting whether it ever was contagious. Leprosy is as hideous and fatal now as when we meet it in the pages of the greek physicians. We hear, it is true, in our day of cases so mild as to be of doubtful nature,¹ but we find these equally in the writings of Gersdorf and Gordon.² It is therefore in accord with sound argument to feel some scepticism with respect to leprosy having changed on a point, about which self-deception is so easy and of such frequent occurrence.

For ages men gave credit to the stories told by lepers about their having noticed, in a few days after a suspicious connexion, the signs which infallibly foretold the coming infection, just as credit was formerly given to the accounts by fanciful women about their knowing by their internal sensations that they had been impregnated. Such symptoms were specifically described by leprous patients and passed current like gospel. The libidinous disposition of lepers, so contrary to everything that later experience has revealed, their boulimia so expressly described by Aretæus, their urine being like that of a camel, the transmutation of other diseases into leprosy, and the existence of a special leprosy poison, conveyed from one individual to another by a process as material as the transfusion of blood, were just as devoutly believed in at the close of last century. On these points the pathology of Hensler is not a step in advance of that taught five or six hundred years ago; yet it would require a large stock of moral courage to broach such doctrines now.

Syphilis first generally known after 1494. — Although I think that the occasional appearance of constitutional syphilis,

¹ *Journal of Cutaneous Medicine*; Vol. 3, p. 75.

² Hensler; *Vom abendlaendischen Aussatze*; S. 177.

long before what is called the siege of Naples, must be looked upon as established, I still see reason to believe that it never showed itself to any great extent before that time, and that its nature, its individuality, was only imperfectly recognized by medical men till it appeared in Italy and Germany, clothed by terror and imagination in horrors worse than those of a pestilence—

“Omni que epidimiâ magis pestifera labes.”

If it had a name it could scarcely be said to have had a pathological status at the time when Charles the Eighth passed the Alps on his way to Naples; I find no valid reason for believing that any of the cotemporary authors mentioned either by Hensler or others had solved this part of the problem. Were there no other evidence on this head, the work of the genoese surgeon, John de Vigo, would suffice to settle the question. It is impossible such a description could have been written except by a man thoroughly familiar with his subject; the delineation of true chancre, the knowledge of the periods of incubation, the tracing of a crowd of symptoms to their true source, the recognition of the protean nature of these symptoms, the graphic accounts of the pains in the bones, all attest the hand of a master. Now de Vigo expressly says that the disease which broke out, at the date just mentioned, throughout nearly all Italy, was till then quite new.

This statement can of course be taken only as referring to the sudden spread of the disease, to the wide recognition of it as a pathological entity. De Vigo could hardly be ignorant of the fact that it had been noticed in a scattered form before 1494, and that the observations of it had been increasing of late years. If he were, we must conclude that he did not know what was certainly known to many others. Irrespective of the testimony of Delgado and Peter Martyr, of that of Schellig, Wimpfeling and Widman, some of which might be remote and half forgotten, Summaripa (1496) fixes 1490 as the year in which it was brought to Italy from France; Baptist Fregosi, improperly according to Després written Fulgoso, (1509), says it had been spreading for two years before Charles entered Italy, and Caspar Torella (1497) places its origin in Auvergne in 1493, the year given according to Wendt

and the Chronist des Saalkreises for its appearance in Denmark and Saxony, and by Mason Good¹ for its extension through Auvergne and Lombardy. "Sir Ulrich Hutten, Knight of Almayn" also tells us² that it broke out in 1493 or thereabouts, but I confess to the most utter distrust in his chronology. He adds according to Beckett,³ in a passage however which has escaped me, that he caught the disease when a child from his nurse or inherited it. The story is utterly improbable, but I suppose its moral really is that syphilis was rather prevalent at the time; for though Hutten wrote only for the public, whom he might justly enough calculate upon not being hyper-critical about dates, he was, I fancy, too shrewd a man to lay himself open to the chance of being convicted of such a gross error as this must have appeared, if the disease had really never been seen before 1494; for as he was born in 1488, the date of his infection could hardly have been later than 1489 or 90.

Although the two questions are quite distinct, yet the sudden recognition of syphilis, which occurred in 1495 and 6, and the rapid appearance of numerous complete descriptions of what had hitherto been touched upon, if noticed at all, in a loose and fragmentary manner, have been accepted as decisive evidence that syphilis was unknown till 1494. Sir Charles Bell informs us⁴ that within a lifetime a hundred works had been issued on the subject of syphilis, whereas none were written prior to the era (1495, 6 and 7), from which all this publishing dates, and at the end of forty years we find this opinion virtually endorsed in several quarters. The influx is significant as to the sudden expansion of syphilis, perhaps also to the increase of facilities now afforded to authors by the growth of printing, but it has no weight whatever against the occasional appearance of syphilis long before the date mentioned.

The discovery of a new malady is sometimes so purely a matter of accident and time, that we can only arbitrarily connect such a fact with its first appearance among mankind. Take for instance the finding of Addison's disease as an illustration in support of

¹ *Op. citat.*, Vol. 3, p. 385.

² *A Treatise of the French Disease.* By Sir Ulrich Hutten, Kt. Revised and recommended to the Press. By Daniel Turner; 1730, p. 1.

³ *Philosophical Transactions*; 1720, p. 49.

⁴ *Institutes of Surgery*; 1838, Vol. 2, p. 229.

this statement. There is I believe nothing to show that any observer, before the time of this distinguished physician, ever noticed the staining of the skin, much less the connexion between it and disease of the supra-renal capsules. Yet unless we assume that it sprang up at the very time when Addison made it known, these phenomena must have yearly passed unnoticed, and certainly unsolved, before the eyes of hundreds of persons. The clue once found, any tyro can recognize the disease and connect the two sets of symptoms, and the difficulty is to understand why so many able men omitted to do so. In much the same way syphilis may often have been seen, even before the earliest of the dates I have given, without securing more than a passing notice; it may have lurked, and what is more there is a good deal of reason to think it did lurk, for years, possibly even a century or two, in Europe, sometimes mistaken for leprosy, sometimes under another and now forgotten name, much for example's sake as sibbens did in Scotland, its connexion with primary sore unsuspected till at last the truth burst upon men's minds.

Swediaur says,¹ speaking of the time of Celsus, "though however these local complaints, so much resembling our present venereal lues, were not marked or observed to be propagated by coition at so early a period, they were a few centuries after, a long while before the lues broke out, experienced and observed to be so by several successive writers; and that these diseases were the very same with our present complaints every unprejudiced reader may convince himself." Keeping in view Swediaur's prejudices, which would not permit him to see anything new or correct in Hunter's description of primary sores, he has stated the fact pretty fairly; the inference however which has been by more than one author drawn from it, that these sores were not contagious or venereal because Celsus does not speak of them as such, is misleading. Assumed ignorance on his part, beyond which the description given by Celsus does not carry us, is a very different thing from proof of their innocent nature, and the argument in any shape fails to show what it is manifestly intended to enforce, the non-existence of syphilis prior to 1494, for this is disproved, almost certainly as regards true syphilis, and beyond doubt as

¹ *Practical Observations on Venereal Complaints*; 1788, p. 7.

concerns chancroid. Besides when we reflect how difficult it often is now, after so much has been done towards elucidating the pathology and genesis of chancre, to determine the most important point about a sore, namely whether it is due to suspicious intercourse, we can readily understand that such a task was onerous enough for those who had nothing but the look of the sore to guide them, and who had no authorities, no inoculation experiments, to fall back upon, and that great allowance should be made for any shortcomings on the part of the famous roman author.

M. Chabaliér modifies the argument. He admits¹ the sores described by Celsus and all others up to 1494 to be venereal, but contends that they were merely chancroid, and that "there is no record in history of the existence of general symptoms prior to 1494." I need scarcely say that M. Chabaliér does not stand alone in his view, yet I must consider it, though substantially correct so far as regards the much earlier appearance of chancroid than of true syphilis, calculated to defeat the object he has in view, and to lay him open to the charge of overlooking all arguments and facts which may tell against his theory, which is of course that of uncompromising dualism. It is however of such importance to the decision of the question about the unity or duality of syphilis, that in the interests of the strictest truth it ought to be denuded of such a dangerous element as exaggeration.

Rarely seen before that Date.—The foregoing reasons then are those to which I appeal in support of the opinion that for years, possibly quite three quarters of a century, before the invasion of Italy by the French, constitutional syphilis was gathering round the more purely local disease and gradually taking root in Europe. But while this conclusion seems quite justified, I think the proofs are equally strong that the affection was till then rare; indeed I fancy the disproportion between evident traces of systemic disease, and the frequently recurring mention of primary sore, is calculated to strike the mind of any person whose attention is called even cursorily to the matter.

The reader will think that a good deal of this arguing is too circumstantial, that there is too much special pleading and fight-

¹ *Op. citat.*, p. 6.

ing with shadows. In extenuation I must urge that these points have also a material practical bearing upon the pathology of syphilis; they are not intended so much merely to establish its antiquity as an abstract historical point, as also to strike at the root of what I believe to be exaggerated and incorrect views respecting the nature of this disease when first generally observed; and such being their purpose, it becomes necessary to put them forward in the most noticeable shape that I can. It is of no use to attack what is thought to be a false view in too mild and tentative a strain; the question is one, not of style and arrangement of topics, but of effecting conviction, and as this is the all important point, it must be carried at any outlay of superfluity and repetition.

At the outset mention was made, that the great problem of the unity or duality of syphilis is to a certain extent wrapped up in its history, and we are now to face this part of the question. It is not an easy one to solve. Either secondary disease existed as far back as the days of Celsus, or the primary sore of his day began, ages after its first appearance, to take on the power of infecting the system, or a new disease was imported at a later date, and in time became so blended with the old one that we can no longer separate them, a state of things unknown with respect to any other disease. The dualists get out of the difficulty easily enough. They say with M. Chabalier that true syphilis, with its long train of constitutional symptoms, is quite distinct from the disease which appeared before 1494; the latter was simply chancroid, and the two have no more to do with each other than gonorrhœa has to do with either of them. But how do those who believe in the unity of syphilis propose to deal with such a problem? They tell us that the dry and purulent sore, the hard and soft, the phagedænic and the tiny follicular chancre, are but so many deceptive semblances taken on by one radically unchangeable type, from which they all spring and to which they all return, and that all these forms of sore may be followed by secondary disease; but they do not tell us anything which solves the riddle in the earlier part of this paragraph, and those who are fond of having matters cleared up might be excused for asking the reason of this silence. I ask and can scarcely ask too

urgently ; if there be but one syphilitic virus, how is it that we cannot connect the primary sores so admirably described by Celsus with any after consequences ?

Ricord's Theory of the Origin of Syphilis from Glanders.—We now arrive at the era of what is so frequently called the siege of Naples, when such an extraordinary outbreak of syphilis in a most malignant form is said to have taken place, but before going into this part of the subject, it will be necessary to examine one or two points connected with the origin of this disease, foremost among which is the conjecture thrown out by M. Ricord,¹ about the possibility of its being the offspring of glanders. The hypothesis has been caught up and repeated by at least two of his pupils, who however do not say what their real opinions are ; my own must be that it is one of those seducing errors which, like Darwinism or animal magnetism, look very plausible till we ask for proofs. Of these M. Ricord does not offer us a vestige. Generally, when a hypothesis of this kind is put forward, it is at least accompanied by something which suggests a possibility, a potentiality, of being right, but here nothing is attempted beyond the statement by M. Beau, that glanders first appeared in the same year as syphilis, and the thread-bare story about the malignity of the latter and its transmission by the air. As to probability, M. Ricord evidently scorns to regard it. The frightfully fatal nature and rapid course of glanders in the human subject ; the suddenness with which it must have changed from so lethal a condition to one which rarely destroys life and runs a long slow career ; the utter want of all evidence that any like change has ever taken place in a disease transmitted from one of the lower animals to man, go for nothing with him, though they really concur to stamp the idea as one of the wild theories every now and then smuggled into existence by dint of conjectures, and ripened into a sickly maturity under the prestige of a great name. But even were all these reasons wanting, dates alone would overthrow it ; for syphilis, as mentioned, undoubtedly appeared before the time, 1494, which M. Beau, the authority invoked by M. Ricord, assigns for the advent of glanders ; a statement, however, diametrically opposed to the opinion held on this subject by one of the greatest

¹ *Op. citat.*, p. 161.

authorities in England, if not the greatest, Mr. Youatt, who says¹ that glanders “has been recognized from the time of Hippocrates, of Cos, and few veterinary writers have given a more accurate or complete account of its symptoms, than is to be found in the works of the father of medicine;” so that syphilis should also, according to M. Ricord, have been known to Hippocrates.

Besides it is pretty certain that the morbus gallicus was rather widely prevalent in the army led by Charles the Eighth; after discarding a good deal of exaggeration enough remains to prove this. But on M. Ricord’s own showing the disease must at first have been glanders, and had this been the case it would have been impossible for men thus affected either to fight or fly. They can struggle on for a while with syphilis, but the other at once prostrates the strongest. Yet there is no evidence that military operations were ever suspended even for a day by any such cause. The pages of Daniel, Roscoe and Prescott contain no allusion to anything of the kind, though an event so important could scarcely have escaped notice. Coupling this silence with the fact, that a disorder, to have attracted notice in such stirring times, must have been rather widely diffused, and during the first ignorance of its nature would, under such circumstances, infect numbers of men, it becomes very doubtful whether glanders itself ever appeared then to any extent on the theatre of war. A form of morbus gallicus, or what was thought to be such, ending fatally at the expiration of a few days, is obscurely mentioned by some of the earliest authors on this disease,² and this may possibly enough have been glanders, as we know that the latter disease has been in quite modern times repeatedly mistaken for syphilis. This happened in the first case I ever saw of the affection, which was diagnosed as secondary syphilis and delirium tremens, the patient, when brought into the Royal Infirmary at Edinburgh, not being able to give any coherent account.

Syphilis in the Bull and Boar.—I think some of M. Ricord’s readers might have furnished him with a better theory. For instance, should time verify the statement made by Dr. E. Andrews, they would have found the materials in it. This gentleman, who

¹ *The Horse.* By William Youatt; 1866, p. 203.

² Hensler; *Geschichte der Lustseuche*; S. 12 and 45.

is professor of surgery at Chicago College, says¹ that three instances of syphilis in the lower animals have been reported to him by a veterinary surgeon in that city. The first was that of a bull who had a chancre on the penis (!), followed by secondary disease; the second was also in a bull, but of much more doubtful nature; the third was that of a boar, supposed to have a primary sore followed by eruptions on the skin. He was also informed by the same surgeon that gonorrhœa is very common in bulls (!)

Possibly this accounts for the pleasant expression of countenance seen in these animals, who may think it is enough to be the slave and victim of man without sharing in his penalties and diseases. Or a bull in far off times, say about the date of the great outbreak of syphilis, when according to von Hutten,² cattle as well as men were attacked by it, may have suffered heavily in his mind from being affected in this way, and having thus acquired a misanthropic look, would certainly bequeath it to his descendants. The reader may think this ill-timed jesting. I reply that it is simply a legitimate extension of the doctrine that beasts have some share of reasoning power, and that, having acquired peculiarities they *must* transmit these to their offspring. I am quite aware that the argument lacks that solemnity which imposes on men, and that many people like to be imposed on, but I believe most sensible persons will admit, that there is nothing more in what I have said than is to be found in the writings of Prichard and a host of others. Reverting now to the point more immediately under discussion, it seems to me that, supposing any facts can be found in support of the statement that the bull and boar suffer from syphilis, we have before us the possibility of this disease being really communicated from one of the lower animals, particularly when the statement of Dr. Andrews is coupled with the announcement in the *Union Medicale* quoted by M. Ricord,³ without date however, that in Italy syphilis has been seen in horses.

¹ *Medical Press and Circular*; 1872, Vol. 2, p. 34.

² *Op. citat.*, p. 7.

³ *Op. citat.*, p. 156.

Importation of Syphilis from America.—I should not have adverted to this rather stale topic, had I not noticed in some recent works statements in which I feel unable to concur, and having gone rather carefully through some of the earlier authorities on this part of the subject, I will now endeavour to put as clearly as I can what appears to me the right view of it.

Astruc tells us, relying principally on the authority of Oviedo, and Ruy Diaz, a physician of Seville, that syphilis was brought by the followers of Columbus to Barcelona, where they gave it to the whole city, so frightening the people that “fasts, religious devotions and alms” were enjoined to propitiate the offended Deity who had thus chastised them. From Barcelona the disease was conveyed by the soldiers under Gonsalvo de Cordova to Naples, where the french soldiers caught it and conveyed it to France, particularly to Lyons by certain “Gens du Roy,” according to the chronicle of Estève de Mèges. Human credulity was rather severely taxed when it was asked to accept a tale which is as improbable as it is untrue. Nor will it avail to say that it is easy enough now for us to judge accurately, for the evidence against the story is as old as the story itself.

Of all the great men who ever lived Columbus was perhaps the least likely to commit the mistake attributed to him. The syphilis of that day is described as eminently disfiguring, prostrating and fatal, and it was going beyond all bounds to tell men that he, who was so wonderfully observant, would have overlooked the ravages of such a disease; that he, who had so much reason to be cautious, who was so continually watched by vigilant and relentless foes and detractors, would take men affected with such a loathsome malady to a city where the “Catholic King” himself was residing. He reached Barcelona with only six Indians and a few sailors. The former, being almost naked, would have exhibited visible traces of the complaint, and the latter must one and all have had syphilis to propagate the infection through even a noticeable portion of so populous a place. Had they been twice as numerous, and had they carried with them maladies so contagious as small-pox and scarlatina, they could not not have infected “the whole city,” and the much greater numbers of them left on the way did not infect Seville and Palos, a difficulty which

Girtanner gets over by saying¹ that Columbus landed at Barcelona ! Cordova, who took the disease from Spain to Naples, only went thither two years and two months after the great discoverer had contaminated the city, during all which time the disease, which created such astonishment and alarm in Italy and Germany almost as soon as it was generally known, must have ravaged Spain almost unnoticed.

Astruc's authorities are worthy of himself. The story of the american origin, though in the shape of a belief as old as the days of Torella² was invented or perhaps re-invented by Leonard Schmaus, a Strasburg, or according to Astruc, Salzburg, physician, of whom Mason Good curtly remarks, that "neither his history nor his arguments are in any degree satisfactory." Oviedo, on whom he so much relies, who was a boy of fifteen when Columbus first returned, and wrote his first work thirty-two years after this event, was treated by some of the best spanish historians of his day—Ferdinand Columbus, Herrera and Las Casas—as a literary Munchausen, the latter declaring that his works are a wholesale fabrication, as full of lies as of pages, a reputation which has not improved at the present day.³ But unenviable as his notoriety might be, he is indebted here to the bad faith and heated imagination of Astruc, who in my opinion was a monomaniac—for I cannot understand any man, really right in his mind, persistently doing such things as he did, to quote no other instance translating "Ethiopia" by "the West Indies"—and from studying whose monstrous work M. Ricord piously entreats God to protect him.⁴ Oviedo indeed maintained⁵ that the disease came from the West Indies, but he referred to 1496, not 1493 as Astruc would have us believe. Astruc puts faith in him when he tells us that as a boy of fifteen he learned that Columbus had brought the disease to Barcelona ; he omits to do so when Oviedo, so punctilious in all matters of religion, is silent about the public fasts and penances

¹ Hensler ; *Ueber den westindischen Ursprung der Lustseuche*, S. 18.

² Chaballier ; *Op. citat.*, p. 89.

³ *History of the Conquest of Peru*. By William H. Prescott ; 1855, Vol. 2, p. 44.

⁴ "Dieu me preserve de le discuter." *Op. citat.* p. 169.

⁵ Hensler ; *Belege*, S. 6.

enjoined to avert the pestilence, about which Peter Martyr too, who was at Barcelona at this very time and for six months after, who witnessed the arrival of Columbus and mentions the immortal navigator and his discoveries in several letters, does not say a word, while he is equally silent in them about the importation and diffusion of syphilis, and in his work *De rebus Oceanicis* about syphilis being found in the West Indies, though he had such excellent means of getting at the truth. As to Ruy Diaz, who wrote sixty-two years after the first homeward voyage of Columbus, he cannot be supposed to speak in any way with authority.

Oviedo seems to have cared little enough about the truth so long as he pleased the ear of his imperial master, and it is therefore really in no way to his credit here that he seems to have come very near the facts, for after all it is not improbable that a certain amount of syphilis was imported from Hispaniola, but too late to save the theory of Astruc from ruin. So early as the spring of 1494 we find the Spaniards at Isabella reported, on excellent authority¹ as suffering from syphilis, which they are said to have contracted from their licentious intercourse with the natives, while I do not find anywhere evidence in favour of the conjecture that this syphilis could at this time have been carried by Spaniards to the Islands. And whether endemic or not, this disease, which spread so slowly in Spain on its first appearance² seems for some reason or other to have raged contagiously in Hispaniola, for Columbus, at his third voyage thither in 1498, is said³ to have found that the hundred and sixty men left, had all got syphilis.

Syphilis in China and the East.—My last reason for touching upon the american origin of syphilis is that put forward by myself a good while ago⁴—the probability that some day or other we might have the story told again, but this time in another form and of another country. A trustworthy witness, Dr. Thomas Nelson, stated before the Committee on Venereal Disease⁵, that in that

¹ *Works of Washington Irving (Life and Voyages of Columbus)*; 1866, Vol. 6, p. 244.

² Hensler; *Ueber den westindischen Ursprung der Lustseuche*, S. 34.

³ *Ibid.*, p. 46.

⁴ *Edinburgh Medical Journal*; Vol. xix, p. 7.

⁵ *Report of the Committee on Venereal Disease*; 1866, p. 111.

immensely ancient country, China, syphilis had existed from time immemorial, and that he had found traces of it in Japan. Now it is true that intercourse between these countries and western Europe, or indeed any part of Europe, might be described almost as non-existent in the fifteenth century; still China had been reached from Italy two hundred years previously, and indirectly through Egypt and Arabia there was communication at least as far back as the days of the early caliphs, seeing that there is no great interval between the time when the Arabs pursued Yesdegird to the confines of Bactria, and that in which they carried their conquering armies into Spain.¹ Thus, through a route like that which the Polos took, syphilis might have found its way to some place of great resort like Constantinople, Negropont or Venice. Or its home may have been nearer, in an equally ancient country like India. Klein says² it had been known for ages in the East under the name of Moecho Wiadi.

Sudden Increase of Syphilis after 1494.—But by whatever means it got into Europe, it seems pretty certain that, at the date just recited, it spread with a rapidity which has furnished only too much food for credulity on the one hand and invention on the other. I will not weary the reader by quoting all the evidence on this head; one or two specimens will suffice. Twenty-six years after its great outbreak it had according to Lemaire spread over the whole world; “Par tout le monde universellement” are the words always quoted. Fracastori, one of the most learned men of his day, writing in the pontificate of Leo the Tenth, tells us that it had extended over Europe and part of Asia and Africa; and Hensler, the historian of syphilis, says³ that it seized upon a sixth part of mankind (befing den sechsten Theil der lebenden Menschen). Chabalier’s figurative language is quite as strong as that of Lemaire, for he observes that the disorder appeared in nearly all ranks of society “almost in the twinkling of an eye.”

I wonder if any person fit to be at large ever believed all this, and if men do not believe it, why is each generation of readers doomed to wade through these mazes of fancy, worse if possible

¹ *Works of Washington Irving (The Successors of Mahomet)*; Vol. x, p. 150, &c.

² Adams; *Op. citat.*, p. 191.

³ *Geschichte der Lustseuche; Vorbericht.*

than the exaggerated figures of speech in which so many historians indulge? Had the disease spread in the way described, it would have brought great part of the business of life to a standstill, and have seriously thinned the population. Let the reader picture to himself the state things would be in, with a sixth part of the population prostrated by a disease so fell as the syphilis of that day is described to be, and lasting for so many years. But exaggeration has always dogged the footsteps of syphilis, and men seem quite content to let it do so. Retrenching however sufficiently to allow for romancing, we may admit that the disease progressed at an unusual rate. Had it died out once for all it might have been taken for an epidemic. M. Ricord speaks¹ of it as such, and sees in its rapid diffusion ground for the theory of its transmission by the air. I presume future ages will rather think that he might have found in such a fact ground for admitting what he so long contested, namely that secondary syphilis is under certain circumstances conveyed by contact, and very quickly too when no precautions are taken to guard against the danger; that this and the ignorance of men on the subject were the reasons why the disease extended so rapidly; and that there was no epidemic in the proper sense of the word, an error justly opposed by so sensible an author as Fournier.²

For, giving due weight to certain contingencies, such as ignorance of the contagious nature of the disease, and the crowding together of troops, I do not know of a single authentic fact which shows that syphilis was communicated more rapidly in 1495 and 6 than has occasionally happened since that time. With a malady creeping up, as it had been doing for some years, nothing was needed beyond some fortuitous gathering of people (a few of them very probably affected with syphilis) such for instance as would be required for the purposes of war, to develop the tragedy, as it has been called, of Rivalta on an amplified scale, with a kingdom for its arena and an army for its victims. Such a possibility will not appear overstrained to those who have read in Swediaur³, that syphilis, in a thinly peopled district like that round the Bay of St. Paul, extended

¹ *Op. citat.*, p. 161.

² Fracastor; *La Syphilis*. Par le docteur Alfred Fournier; 1870, p. 44.

³ *Op. citat.*, p. 173.

so fast that in 1785, when the observations he relies on were made, 5801 persons were known to be suffering from it, besides many who concealed the fact; and though he is sometimes very inaccurate, I fancy we should be safe in admitting the number to have been large. Nor is this a solitary fact. Haeser gives¹ two instances where syphilis, in the eighteenth century, spread with great rapidity, an outbreak at Zurich being so bad that it is spoken of as a raging disease (*grassirende Krankheit*).

The french Army not the sole Medium of Diffusion.—Ever since the time of Sebastian Brant, 1496, and of Conrad Gilinus, 1497, an opinion has prevailed more or less extensively, and is again adopted by one of the most recent authors² from Simon, that it was the soldiers of Charles the Eighth who took syphilis with them into France and Germany, having of course caught it in Naples, whither the disease had been transported from Spain. Although some writers, who ought to be authorities, contradict each other a good deal here, the predominant theory seems to be that the French are to have the credit of sowing syphilis so broadcast, and I therefore propose to deal with it. That many of the soldiers were infected is probable, but there are good reasons for considering that an extreme view has been taken of the mischief they disseminated. The [number of men did not, at a fair computation, exceed twenty thousand when Charles left Asti on his road to the south of Italy.³ The soldiers supposed to have been principally instrumental in diffusing the disease were the Swiss and Germans, and of these only six thousand, chiefly Swiss, started for the seat of hostilities,⁴ and only twenty-five hundred left Naples on the homeward march, many probably remaining there, as only part of the army was to return. Indeed the whole body of troops, which then prepared to quit Italy, did not number more than nine thousand fighting men,⁵ who were most seriously thinned down by the battle of the Taro, the painful retreat and the hardships they endured.

Those left behind got on still worse. Of five thousand men

¹ *Op. citat.*, Vol. 2, p. 291.

² Auspitz; *Die Lehre vom Syphilitischen Contagium*; 1866, S. 29.

³ *Histoire de France*. Par le Père Daniel; 1742, Tome viii, p. 595.

⁴ *Ibid.*

⁵ Prescott; *History of Ferdinand and Isabella*, Vol. 2, p. 39.

who marched out of Atella not more than five hundred ever reached their native country. Upwards of four thousand more perished in the Isle of Procida,¹ and Després says,² that out of the army left under the command of Gilbert de Montpensier, which a short time before (swelled I suppose by reinforcements) at Salerno amounted to nineteen thousand, scarcely three hundred re-entered France. We may therefore believe the story that not more than one-fourth part of the original army ever got back, and if the statement of a much respected author, Fallopius, that the whole french army (*ferè omnes*) was infected with syphilis,³ be correct, even this number must have escaped from Italy by something like a miracle. The Swiss and Germans were in as bad a plight as any. "They made their way as they best could through Italy in the most deplorable state of destitution and suffering."⁴ By a passage which Haeser quotes⁵ from Meyer Ahrens, we learn that the Germans and confederated mercenaries, particularly those whom the king left behind at Naples, were in a frightful condition. Those, it tells us, who did not fall by the daggers of the Italians, who did not perish as solitary stragglers of hunger and thirst, or by poison, in barns or fields, by the roadside or on a dunghill, were so wasted as hardly to be recognized by their friends.

South Germany and France, if not Switzerland also, were then somewhat thickly peopled and thriving countries, inhabited by highly gifted races of men, and advanced in culture. That the broken remnants of a small army disseminated a complaint, which must have made them objects of abhorrence to every beholder (and which, had it been the dire malady described by so many authorities, not unfrequently fatal, and from which according to Sabellicus few people recovered, would have invalidated them to the last man before they re-crossed the Alps), so extensively as to fill whole kingdoms with syphilis, is I submit, to levy a rather extortionate tribute upon our easy belief. Yet unless I have quite misunderstood some modern authors, this is what they mean,

¹ *Life and Pontificate of Leo the Tenth.* By William Roscoe ; 1806, Vol. 1, p. 361.

² *Op. citat.*, p. 39.

³ *De Morbo Gallico* ; 1563, p. 1.

⁴ Jovius. Quoted by Prescott. *Reign of Ferdinand and Isabella* ; Vol. 2, p. 61.

⁵ *Op. citat.*, Vol. 2, p. 325.

and what their authorities mean ; for instance Gruenbeck expressly says that it had made its way into every part of Germany.¹

The remark, that this disease must have made these wretched soldiers objects of abhorrence, requires some explanation. We read that, at the times when these scenes were passing before men's eyes, a patient affected with syphilis emitted an intolerable stench, stank worse than a monk of the olden time ; that he was emaciated as if by famine, and that he was covered with scabs "from the skull to the knee-cap," the forehead, nose and ears being studded with great rupial crusts like "little staves, horns and teeth" in shape, such being at any rate the plight the Italian soldiers were in. A worthy Alsatian priest of that day (1510), who chronicles the popular belief that this was the disease with which "the devils" (*die Tuffel*) plagued Job, improves so far upon the foregoing description as to tell us that the growths were as long as the joint of a man's finger. Even the well-to-do, if known to be infected, were shunned by their friends, and it was considered a testimony of devoted attachment to hold intercourse with creatures so marked by the curse of Heaven as the venereal. I do not say that all this or any part of it is true, but such is the description, and I ask the reader how he proposes to receive the idea of men in this state being generally admitted to such close contact with people as to diffuse this malady on every side. But the more narrowly we look at the question, the more strongly does the suspicion take root in our minds that the story of the French catching the disease at Naples is a piece of invention, to the circulation of which Ulrich von Hutten, though he does not restrict himself entirely to this theory, materially contributed by the great popularity of his work. Most of the earlier authors, who wrote about the time of this event, do not, as Beckett puts it,² "say one Word about the Neapolitan Story," that is to say of the spreading of the *morbus gallicus* having "had its Rise from the French Soldiers' Conversation with the Italian Women."

Great Severity of Syphilis at its first Outbreak and subsequent Decline. — With few exceptions writers on this disease have

¹ "per totum Germaniæ tractum, urbes, oppida, castra, pagos et villas."

² *Philosophical Transactions* ; 1720, p. 47.

affirmed that, when it first broke out at the close of the fifteenth century, it was, and for some years after continued to be, of a far more formidable nature than at present, eating deeply into the flesh and destroying the bones extensively,¹ besides signaling itself by the presence of some malignant symptoms previously recited. It ran its course too with alarming rapidity, change of colour of the face and great depression of spirits coming on within three or four days after infection.² Haeser indeed thinks³ we are justified in believing that the interval between the appearance of the primary sore and that of the skin disease was shorter than now. The disease was so infectious as to taint, not only the air of the house but even the trees and plants, particularly the vines and cabbages.⁴ After the complaint had for a few years alarmed and astonished the world, it underwent a singular decline, or as we find it put in Turner's translation of von Hutten, "gradually abated of its Fierceness." According to an incomprehensible statement of Hensler,⁵ the real old fierce pestilence of syphilis died out altogether, so that what we recognize under that name has nothing to do with the epidemic. Were this story about the malignant nature of syphilis at the outset merely an old belief, I should put it down as one of the tales incident to the subject, but it is too generally current for that. For instance, if we take the men who may be considered as fairly representing their respective countries in this department, Lee, Ricord, Bumstead and Haeser, we find them all pledged to this view of the case, the last named author perhaps not quite so markedly as the others.⁶

Mr. Lee, the first living authority in England on syphilis, quotes⁷ Fracastori as witness on this point, the following being the passage selected.⁸

"Protinus informes totum per corpus achores
Rumpebant, faciemque horrendam et pectora faede
Turpabant: species morbi nova, pustula summæ

¹ Haeser; *Op. citat.*, Vol. 2, p. 237.

² *Ibid.*, p. 229.

³ *Ibid.*, p. 231.

⁴ Hensler; *Belege*, S. 9.

⁵ *Vom abendlaendischen Aussatze*; S. 230.

⁶ *Op. citat.*, Vol. 2, p. 187.

⁷ *Syphilitic and Vaccino Syphilitic Inoculation*; 1863, p. 152.

⁸ Hieronymi Fracastorii; *Syphilis*, 1536, L. 338.

Glandis ad effigiem et pituita marcida pinguis :
Tempore quæ multo non post adaperta dehiscens,
Mucosa multum sanie, taboque fluebat.
Quinetiam erodens alte, et se funditus abdens
Corpora pascebat misere, nam sæpius ipsi
Carne sua exutos artus, squallentiaque ossa
Vidimus, et fœdo rosea ora dehiscere hiatu,
Ora, atque exiles reddentia guttura voces.
Tum saepe aut cerasis, aut Phyllidis arbore tristi,
Vidisti pinguem ex udis manare liquorem
Corticibus, mox in lentum durescere gummi.
Haud secus hac sub labe solet per corpora mucor
Diffluere : hinc demum in turpem concreescere callum.
Unde aliquis ver ætatis, pulchramque juventam
Spirans, et membra oculis deformia torvis
Prospiciens, foedosque artus, turgentiaque ora,
Saepe deos, saepe astra, miser crudelia dixit.
Interea dulces somnos, noctisque soporem
Omnia par terras animalia fessa trahebant :
Illis nulla quies aderat, sopor omnis in auras
Fugerat : iis oriens ingrata Aurora rubebat :
Iis inimica dies, inimicaque noctis imago.
Nulla Ceres illos, Bacchi non ulla juvabant
Munera non dulces epulæ, non copia rerum,
Non urbis, non ruris opes, non ulla voluptas.”

This passage may, I think, be fairly translated as follows.
“Straightway filthy pustules broke out over the whole body, disfiguring the face and chest in a revolting manner; a new species of disease. The pustule, which was much like the top of an acorn, and full of heavy phlegm, soon gaped and poured forth a quantity of mucous sanies and gore. Then making its way inwards it preyed grievously upon the frame. But oftener still we saw the limbs stripped of their flesh, and the repulsive bones, while the mouth gaped with a horrible opening; the (state of the) mouth making the voice shrill. Then, often, as thou has seen in the cherry or sad Phyllis’s tree (the almond), the gross fluid distil from the moist bark and the gum slowly harden, even so under

the power of this foul sickness was the mucus wont to flow from the body and thicken, in time, into nasty crusts. Thus a miserable sufferer, in the spring of life, sighing after delightful youth, now grimly regarding his deformed limbs, his loathsome frame and swollen mouth, would upbraid, sometimes the gods, sometimes the stars, with cruelty. While every wearied animal on earth enjoyed the privileges of sweet sleep and the stillness of night, there was no peace for these victims of misfortune, and slumber fled from them. For them Aurora dawned unwelcome, and night came in the likeness of a hideous spectre. No delicate food nor gifts of Bacchus availed them, nor pleasant feasts or plenty, the wealth of the city or country, nor any kind of pleasure."

Such is the account given by this "most learned of men," this "mighty physician and poet," "*medicus ingens ingens que poeta.*" Of the poetical merits of the "*Syphilis*" I do not profess to be a judge. A great modern scholar, Dr. Parr, has pronounced it to be "nearly equal to Virgil,"¹ and I bow to his decision. But as a piece of medical evidence I say at once that this part of it at least will not bear looking into. Everything conspires too, to prove that, if Fracastori ever saw the disease at all, he did not do so till long after it had "abated of its Fierceness," and that he was not an eye-witness of the dire symptoms he has described. An excellent scholar, Roscoe, considers that the date of his birth may be fixed "with tolerable certainty" in 1483. Consequently he could only be eleven years old when the great outbreak of syphilis began; indeed it is interesting to notice that some of the most startling narratives date long after the events chronicled, such as that of James Bethencourt, 1527, and Lawrence Phrisius, 1532, though it must be admitted that the accounts by Gruenbeck, 1496 and 1503, and Sabellicus, 1502-9, are highly enough coloured. Up to the date of the invasion of Italy, and for long after, Fracastori resided in the north-east of the kingdom, at Verona and Padua, far away from the chief scenes of war and pestilence; so that we may entertain grave doubts as to whether he ever met with more than a stray case

¹ *Memoirs of Thomas Moore.* By Lord John Russell; 1853, Vol. 2, p. 147.

of syphilis before the date of the battle of Ghiarandaddo, 1509, after which he returned to Verona and devoted himself to literary and scientific pursuits.

Men familiar with his biography will, I fancy, admit that he could not have cultivated practical medicine to any great extent, for like many in that classical age he gave up a large portion of his time to other studies. To be a profound scholar and a proficient "in mathematics, in cosmography, in astronomy and other branches of natural science," demands so much expenditure of time as to leave but a scanty residue for the investigation of disease. Those who can swallow the fables told, under the guise of biography, in the lives of Pico de Mirandola and the admirable Crichton, may believe that a youth of genius, who has mastered several branches of learning, may also be a great physician, and, what is more to our purpose, have found time to examine carefully the course of a malady which has tried the powers of so many famous men; but the common experience of our profession has decided, that a jealous and absorbing art like medicine suffers no intruder near, and that he who would excel in it must relinquish all hopes of celebrity in other branches of knowledge.

Farthermore it is likely that Fracastori did not practise in a way likely to yield any results worth notice. Among his merits was that of exercising his practice gratis (*citra lucrum*), which, I suppose, means, when reduced to plain terms, that he was a mere dabbler, and scarcely better fitted to give an opinion than the benevolent curate of some country district, who, having read through Buchan, concludes that he can now minister to the bodily as he does to the spiritual maladies of his flock, and forthwith proceeds to act as medical adviser. Lastly the famous physician and poet seems to have had but a slender acquaintance with the standard medical authorities on the subject, as is evidenced by the great discrepancy between his views and theirs. Indeed he is far behind the best of them, and neither as a pathologist or a practitioner can be said to equal Leonicens, Torella or de Vigo. His looseness of expression, even in points where he is supposed to have been such a proficient, and where he might so easily have arrived at accuracy, is startling. For instance he says that syphilis broke forth *about* 1490, at the time when the French under

Charles the Eighth occupied the kingdom of Naples, though all the world knows that this took place quite four years later.¹

We identify the disease as it appears in his poem by the name he gave it and the narrative of its outbreak, not by the similitude of the symptoms with those set forth by any trustworthy author of his day. The description just quoted is superficial, as is also that in his medical work, and, for medical purposes, incomplete, seeing that though there is a beginning, there is no end to it, and that we can only guess at the previous career and subsequent fate of the imaginary sufferer. But though it is intended to awaken horror, I cannot see that the severity of the complaint portrayed exceeds that of the malignant syphilis spoken of by Mr. Walter Coulson,² the acute secondary ulceration of which Dr. John Morgan speaks,³ or the eruption like rather confluent small-pox mentioned by Basereau;⁴ descriptions which should be contrasted with the statements of Torella and Beniveni, that pustules did not preponderate in these early days,⁵ and that of Tomitanus⁶ that pustules scarcely appeared in his time. It is difficult to understand how any disease could be much worse than that described by the modern writers just quoted, and if we were told the contrary on even much better evidence than that of a poet, I should still feel sceptical. For inasmuch as authority and tradition are powerless to change the laws of nature, so they cannot claim a hearing when arrayed in support of anything which premises a gross infringement of these and violates probability. It is violated when we are taught that men languished for years under a disease much worse than the worst syphilis of the present day, such as we understand that of Fracastori was. A disease so frightful would have made short work with its victims, and the patient would not long have endured the misery of contemplating his fleshless limbs, seeing that death would have speedily relieved him from any such task, and have spared the physician the task of looking upon his "repulsive bones." But if the reader still think the old version the right one,

¹ "in italiam vero ferè iis temporibus erupit, quibus Galli sub rege Carolo regnum Neapolitanan occupavere, annos circiter decem ante 1500."

² *A Treatise on Syphilis*; 1869, p. 141.

³ *Practical Lessons on the Contagious Diseases*; 1872, p. 157, 229.

⁴ *Op. citat.*, p. 418.

⁵ *Ibid.*, p. 5.

⁶ Haeser; *Op. citat.*, Vol. 2, p. 269.

it will be a satisfaction to him when he knows that these afflicted people kept up their appetites and even gave away to a little gluttony.¹

Leaving out of sight the exceptionally bad cases, does Fracastori, either in his poem or his later work, make the disease worse than the realities of everyday life? Neglected and ill-treated syphilis was always and is still a formidable and repulsive malady, and no one can be surprised to learn that, when it had not been energetically met, there were bone pains so severe as to prevent sleep, and bad suppurating tubercles. Besides Fracastori gives testimony against the severity because he does so against the decline of it. He wrote at a period much later than that assigned to the improvement in the character of the disease, yet he says,² not that it had ameliorated but that it had altered, there being now for the last six years scarcely any eruption and almost no pains or very slight ones, but numerous cases of gum knot, there being no mention that they are rare,³ and certainly the present age has not improved in this respect. The last part of the assertion looks like carelessness of expression, gum knots having been long before described by so well known an author as de Vigo, in words which an old translator renders as "certain knobbes of grosse and phlegmatike matter." It is quite natural that there should be little or nothing said about gum knots in the earliest writers, for the reason that these growths had not then had time to show themselves. And granting anything so improbable as the assertion about the bone pains to be correct, we must conclude that syphilis is now very much worse than it was; indeed according to Fernelius⁴ the osseous pains and gummata were again bad in 1557. But it is very questionable whether Fracastori ever thought much about accuracy, and whether he did, or cared to do, more than reflect, in elegant and classic Latin, the opinions on syphilis current among the scholars of his day. The poem

¹ Pinctor; *De morbo foedo, &c.*

² Hieron. Fracastorii; *Operum Pars prior*; 1591, p. 179.

³ "Porro et annis labentibus, annis jam ferè vi in quib. nunc sumus, magna rursus mutatio jam facta est ejus morbi: quippe quam in valde paucis pustulæ jam visantur, et dolores ferè nulli aut multo leviores, gummositates vero multæ."

⁴ Haeser; *Op. citat.*, Vol. 2, p. 270.

especially looks as if he were more intent on writing like a scholar and a gentleman than a physician.

Some authors, referring to the change in the nature of syphilis mentioned by Fracastori, tell us that alopecia first appeared in his time. I am disposed to think that the passage on which this opinion is based means something very different ; assuredly he was not thinking on what we constantly speak of as a sign of constitutional infection. First I contend that the passage shows Fracastori's ignorance practically of the disease, for, from the way in which he describes the symptom, I should say that he had confounded tinea decalvans in a syphilitic subject with the effects of syphilis, though it might be the affection Bassereau speaks of,¹ which is unknown to me. All the medical evidence of that day is opposed to the surmise that total loss of the hair was a common sequence of venereal disease, and though we are told that at one time the beard was in some places cherished as a sign that the wearer had not suffered from the dreaded complaint, this only shows that an occasional circumstance has been magnified into a rule of pathology, and that a superstition, such as that upon which this ceremony of wearing the beard reposes, is more easily invented than overthrown. Besides Fracastori² tells us that the teeth dropped out, and that this was not owing to the mercury but to the disease, a part of his narrative which certainly needs confirmation. In the same way I believe the accounts about the stupor, which is said to have, in the infancy of syphilis, preceded the outbreak of the constitutional disease, and which Fournier tells us³ he has noticed, are to be explained ; a fortuitous occurrence being expanded into a sign of almost pathognomonic value.

One word about the osseous pains under which the earlier sufferers from syphilis laboured, and which have so often attracted notice. To judge from the language made use of, these torments ought to have been something dreadful, and one might think the victims of them went through a martyrdom which would have speedily broken down our less robust frames. The older writers constantly expatiate on this theme, and I suppose I should be within bounds if I were to say that the tale has since been re-told

¹ *Op. citat.*, p. 74.

² *Operum Pars prior*, p. 180.

³ *La Syphilis* ; p. 50.

a hundred times. Yet if we can repose faith in the statements about the quickness with which such pains yielded to simple means, we must believe that the descriptions were indeed painted "in lively colours." For instance Pinctor speaks of the pains as shifting their seat, and says that when inunction was carried out they were relieved in four to six days, and the patients were completely cured of them by the end of the eighth day. I give his own words for this.¹ Are we then really to understand that wandering pains, which could be cut short so summarily, were ever of such severity as to justify the impression which has been taken up about them?

As to the more rapid evolution of syphilis at its first appearance, the evidence seems to amount to this: There is no doubt that some few cases of this kind are reported, very briefly and imperfectly, or only just alluded to; still, most probably with substantial accuracy. But I need scarcely say that in the present day instances of hasty evolution are occasionally to be found. One of Torella's five cases is frequently cited² as conclusive evidence respecting the more rapid march of syphilis at the very outset, but, with due consideration to the less attention then paid to dates, I see little difference between the nature of the disease mentioned and that detailed by Dr. John Morgan³ and by Bassereau;⁴ while there is quite as good evidence that the complaint usually, if not always, proceeded at the same rate in the fifteenth as in the nineteenth century.⁵ There being then, in my opinion, no proof of greater malignity at the outset, the story of the decline needs no refutation, for that which did not exist could not abate.

But whether Fracastori's evidence was for or against the common belief, I would equally banish it as I would everything like it. If we are to get at the truth it will only be by excluding everything not thoroughly trustworthy, and under this head we

¹ "in una hora in capite, in alia hora in tibiis et brachiis etiamque in musculis" "sed post, transactis 4 diebus vel 6, quieti e doloribus fuerunt et pustulæ omnes remotæ; et sic continuando ipsas unctiones in 8 diebus a doloribus fortibus sanati fuerunt."

² Chabaliér; *Op. citat.*, p. 91. ³ *Op. citat.*, p. 104.

⁴ *Op. citat.*, p. 164. ⁵ Torella; Gruenbeck, *De Mentulagra*, &c.

cannot rank the effusions of an author who was a poet, not a physician. We may perhaps confide implicitly enough in poets, when they describe the spirit of their times and the springs of human passions, but in pure matters of fact the greatest of them are not to be trusted. Very likely Homer faithfully reproduced the manners and customs which prevailed in the grecian age of bronze, and Shakspeare those current in the days of Elizabeth ; but, to select only two out of almost countless instances, the most credulous schoolboy never believed that Achilles leaped “far as a spear can fly,” ὅσον τ’ ἐπὶ δούρῳς ἐρωή, nor does the rudest seaman require to be told, that the ocean does not “mount the welkin’s cheek” and dash out the fires of heaven.

I would mete out the same measure to von Hutten, who seems to have been, if not the founder, at least the chief apostle of the creed about the decline of syphilis at the end of seven years, and from whose work Fracastori possibly drew some of the materials for his description. It is certainly calculated to excite a suspicion of this kind, when we compare those passages in the poem on syphilis relating to what are evidently considered to be the most salient features of the disease, that is to say the filthy pustules, the wasting and the night pains, with Hutten’s account. “They had,” he says,¹ “Boils that stood out like Acorns, from which issued such filthy stinking Matter,” &c. ;² and again, “The Sick grows lean, his Flesh wasting away, so that there remaineth only the Skin as a Cover.” What tends to confirm the surmise is that at a later date Fernelius repeats almost the words of von Hutten.³

From whom this restless mortal derived his facts we are left to conjecture, as he does not say a word on this head, and judging from the way in which he speaks, with one or two exceptions, of medical men, he was not likely to trouble them for information. Most assuredly we should seek in vain for even an idea of the opinions held by the leading physicians on the subject of his

¹ *Op. citat.*, p. 3.

² The words in the original are “Ulcera in quernæ glandis speciem et magnitudinem, aspera, exporrecta, spercus ab his profluens humor.” *De Guaiaci Medicina*, Caput I.

³ Haeser ; *Op. citat.*, Vol. 2, p. 272.

treatise, and he, who had no better authority than von Hutten to guide him, would form a very inadequate estimate of their labours and their merits. So far as I have been able to make out, the only author of repute in that time, who even expresses an opinion about this sudden mitigation of syphilis, is de Vigo, and he merely speaks¹ of its being less contagious than at first; an opinion possibly founded on the fact that in its earlier days men thought the disease was epidemic, and did not take so much pains to avoid contact, or, still more probably, on exaggerated stories put in circulation during the first period of alarm.

Judging from the work just quoted, and from a careful perusal of his biography,² I am inclined to view, not only von Hutten's opinions, but a great deal of the often told story about his eleven salivations and cure, with the greatest scepticism, or perhaps it would be more straightforward to say, that I believe one part of the narrative to be exaggeration and the rest of it fable. In the first place it is almost certain that he never saw any cases but his own and his father's, and of the latter very little indeed, as he only visited home once or twice after he had himself contracted the disease. Of the great outbreak of syphilis in Italy and Germany he could have seen nothing, for he was born in 1488, and all the worst features of the so-called epidemic had, according to his own express statement,³ passed away by the time he was twelve years old. He opens his book on syphilis with an error, or at least a vagueness, calculated to shake all faith in his accuracy, for he says that the disease broke out in 1493 or thereabouts, and in the french army at Naples, whereas Charles the Eighth did not start from Asti till the sixth of October, 1494; nor is this a mere misprint in the figures, for the date is written. He does not seem to have known that the popular name of syphilis, the sickness of St. Mævius, really belonged to leprosy, and was a mistake either on his part or that of the vulgar whose opinions he copied. That he ever studied the disease is simply impossible. A man who was perpetually quarrelling and agitating, rambling and writing, and who at his early death left behind him seventy-two works,

¹ Fol. clx.

² Ulrich von Hutten. Von David Friederich Strauss; 1871.

³ "Neque enim septimo multo annum supra ejus grassatura fuit."

many of no contemptible length, could have had little time to spare for such an absorbing task as the investigation of syphilis. His book on the subject bears every mark of a hasty production, written only for a popular purpose. The excellent biography of him by Strauss, as just mentioned, makes his eleven courses of mercury something more than doubtful ; and his guaiacum treatment, instead of curing him to the confusion of those impudent pretenders, the physicians, really failed to remove the disease, which, after a seeming improvement, carried him off in his thirty-sixth year.¹

Interesting, therefore, as his work is and always will be to the medical scholar and antiquary, I must enter my protest against ranking it in the same class with the writings of experienced physicians of his day ; who, however low some of the moderns may rate them, cultivated their profession with honourable industry, truthfully observing and noting down numbers of facts calculated in their opinion to advance science and improve treatment.

From Mr. Lee² we learn, that the outbreak of syphilis at Rivalta was accompanied by an eruption of so-called pustules, as a result of which it was in some instances confounded with smallpox, and that the same thing happened when the disease appeared in Europe at the close of the fifteenth century. As Mr. Lee does not quote his authority we must take the case on his own showing, which we may very safely do. The occurrence is probable enough. Secondary pustular eruptions, when copious and occurring at an early date, accompanied by feverishness, have been rather frequently than otherwise mistaken for smallpox. I have myself seen two instances of this error, which indeed has occurred often enough to need no particulars in the way of proof. But, in such case, what becomes of the theory about a decline in the severity and a change in the character of syphilis ? Tried by this test, how can either have happened if the disease reappear in 1861, with such a serious symptom as syphilitic ecthyma attached to it in the same guise as on its first reputed outbreak ? A visible and tangible disorder like syphilis would be apt, one might think, when it grows milder, to change its look at the same time. It

¹ Strauss ; *Op. citat.*, p. 533.

² *Syphilitic and Vaccino Syphilitic Inoculation*, p. 154.

may be said that the complaint resumed its old severity at Rivalta because it broke out in a new country ; in that case every little outburst of syphilis, in hitherto uncontaminated places, ought to take on the features which the terrible epidemic wore to the eyes of Gruenbeck and Fracastori. But I am prepared to go beyond mere reasoning, and say at once of this special symptom, that I do not think it could have been worse than we sometimes see it now. I attended a case where the number of pustules was so great that it might be spoken of as enormous ; Bassereau says¹ he has seen persons whose whole skin was covered with the pustules of syphilitic ecthyma, and Dr. John Morgan, at so recent a date as 1872, says² that he had under him a case where the pustules appeared as a first rash, and were so thick that “the finger’s point could hardly be laid on a part of the body clear from the disease.”

If, then, we shut our ears to the fables told by romancers and poets, and confine the evidence to that of persons practising medicine, I think we shall find that the foundation for this superstructure of a disease appalling beyond conception, invading whole kingdoms at a time, and divesting itself of its terrors at an epoch so congenial to superstitious notions as the end of the seventh year, melts into air ; and that, keeping in view the want of influence exerted by proper treatment, there is nothing to warrant the belief that the disease, as pictured by de Vigo or any reliable authority, was worse than, or materially different from, what we may often see now in a Lock hospital.

This refers distinctly to the constitutional effects of syphilis. Of the primary sore we do not have so much in the way of exaggeration. It seems, however, that phagedæna and sloughing prevailed in the french army. The formidable look and intractable nature of these, especially the former, were well calculated to awaken terror in both patient and surgeon. As to sloughing, we may be pretty sure that wherever large bodies of men get together, and when we find privation, fatigue, and debauchery doing their fell work among persons exposed to the contagion of syphilis, there we shall have sloughing. Such has ever been the story. The accounts given of the ravages of syphilis in Lithuania

¹ *Op. citat.*, p. 418.

² *Op. citat.*, p. 149.

and East Prussia after the Seven Years War ; of the same disease in 1806, and again in 1807 and 8, in Berlin after the heavy losses by the Prussians,¹ and numerous reports by army surgeons, corroborate this assertion, even when they ascribe² such malignity, on what I consider imaginary evidence, to climate. With the return to better quarters and food, to more quiet of body and mind, comes a diminution of the evil, and possibly some such change was one reason why the morbus gallicus was supposed to have “abated of its Fierceness,” after the ill-starred attempt of the French on Naples. Be this as it may, we may feel pretty sure that primary sores, much worse than the Black Lion or Swan Alley Chancre, did not prevail to any great extent in their army, or the soldiers would have dropped out of their ranks by tens and twenties at a time.

One piece of evidence on this head deserves special notice. It is that about primary syphilis as it appeared in his own person, given by Gruenbeck or Gruenpeck, for the editor of the latest edition, that I have seen, of his first production, with a sublime contempt for orthography, spells the name at one time with a p and another with a b, who gravely relates³ that his penis, in a brief space of time, say half an hour, swelled to such a size that he could scarcely clasp it with both hands, and that a thousand fistulous passages formed in the swelling, which for nearly four months poured out a continual stream of filthy ichor. He also gives a description of the complaint in its secondary stage, as he saw it, which I place here in juxta-position, that the reader may be enabled to form his own opinion about the value of evidence so often quoted. Gruenbeck says that some of the victims of this disease had, on the forehead, neck, breast, &c., crusts much harder than the bark of a tree ; in some every bone was laid bare ; while others displayed such a multitude of warts and pustules that they could not be counted with anything like accuracy, and others again passed forty, sixty and even a hundred days without sleeping. No wonder that his friends fled from him “as if the naked

¹ Simon ; *Op. citat.*, p. 48.

² *Medico Chirurgical Transactions* ; Vol. 4, p. 1.

³ Libellus de Mentulagra. Quoted in the Excerpta by Hensler, whose reading of the author's name I have adopted.

swords of their enemies hung over their necks," when they heard that he had been attacked by such a malady ; no wonder that those attacked by it could " neither stand, walk nor do any kind of human labour."

This "venerable man" as he is called, though some passages in his life are calculated to make us suspicious about his claim to such a character, ought to have lived a little later and accompanied Baron Munchausen as travelling secretary ; for unless we excuse him on the ground of insanity, we must convict him of gross exaggeration. The human penis does not swell to such a size from any such cause, and no medical author of that time noticed swellings of this magnitude and suddenness. There is not space on the generative organs for a thousand ulcers. No man ever had crusts on him harder than the bark of a tree, and equally no man ever passed the tenth part of a hundred days without sleeping. But even if the narrative had been a good deal more in accord with common experience, I should, conformably with the law of argument adopted when speaking of Fracastori and von Hutten, still object to it as authority on any disputed point in pathology, on the ground that Gruenbeck was not a medical man,¹ but an excitable lay mortal, or rather a secretary subsequently turned priest, more than half crazed by his fears, and not improbably a little touched upon astrological questions and the intentions of the Deity ;² whose remarks about physicians and surgeons, and indeed whose whole story, would lead one to think that he had never seriously attempted to master the subject he was so desirous to enlighten the world about, namely the proper treatment of syphilis, which the medical men of that day understood quite as well as he did. However his testimony is useful in a way he little thought of, for when we lop off the exuberances of fancy, and get at something like the naked truth, we find that the periods of incubation in his case were much the same as in the present day.

Allowing that the primary symptoms had improved at the time Hutten speaks of, they must subsequently have undergone a

¹ Hensler ; *Geschichte der Lustseuche* ; S. 18.

² "præcipue quia divinitus ordinatum est, quod soli rustici et barbari hunc morbum curare possunt."

bad relapse, for Rosenbaum gravely asserts,¹ that towards the close of the sixteenth century a spanish army surgeon amputated the penis five thousand times within three months for this complaint ! There exist no means of knowing whether any person ever believed this monstrous statement ; Simon, however, from whom I borrow the story, gives it without farther comment than that five hundred would have been enough, so that it has passed muster, and may serve as a specimen of the way in which some men write about the history of syphilis. I should have put it down as a fable passing all bounds, for common sense at once assures us that in the most despotic country in the world, the patients would, in self-defence, have destroyed such a dangerous madman as this surgeon. Assuming however that about a hundredth part of the tale is true, the case still does not look like one of improvement.

Possible Complications of Syphilis in 1495, 6 and 7.—Mention has already been made, that glanders not at all improbably played some slight and brief part in the opening scenes of syphilis, and it is by no means impossible that in some instances, the boils spoken of by von Hutten and Fracastori may really have been furunculi, such as have often appeared after epidemics. The cholera of 1849, it will be remembered, was followed for some years by an eruption of this kind, the boils sometimes looking like a string of sloughs, and at others very closely resembling impetigo rodens, from which it was not at all easy, without the history of the case, to distinguish them.

We are told by Alexander Benedict that, in some instances of the morbus gallicus occurring about this time, the hands and feet of the sufferer dropped off. But for one solitary piece of evidence I should have said that this, supposing it really happened, could not have been the work of syphilis, and was more likely to have resulted from gangrenous erysipelas, a disease which appears to have committed fearful ravages of this kind in the good old times. In a quotation by Pereira² from the works of Sigebert we learn that in 1089, which the old chronicler calls “a pestilent year,” this malady, possibly due as has been thought to the use of

¹ Oppenheim's *Zeitschrift* ; B. xiv, S. 471. Quoted in *Ricord's Lehre*, S. 72.

² *Elements of Materia Medica* ; Part 2, 1840, p. 595.

spurred rye, prevailed extensively, and that the victims of it either perished miserably, or, deprived of their putrid hands and feet, were reserved for a more miserable life ;” while in an account given nearly seven hundred years later by Dr. Wollaston of Bury, of apparently the same disease in a modified shape, we are informed that the limbs of several persons attacked by the complaint rotted off. The confirming evidence just referred to is that of Swediaur, who says¹ that the same thing happened, though to a limited extent, when syphilis spread so widely in Canada during the last century, one little boy having lost both feet by the complaint, and the leg having dropped off at the knee in another.

M. Després considers that the often quoted passage in Marcellus Cumanus, about what he saw on arriving at the camp at Novara, should be referred to itch. The account given by Cumanus is that he found several cavaliers and men-at-arms suffering from pustules on the face and all over the body, which began like millet seeds under and outside the prepuce. These pustules “due to an ebullition of humours,” and what I suppose we must translate as “heavenly influences,” *ex uno influxu cælesti*, sometimes appeared without pain but accompanied by itching. Then the patients scratched themselves and ulceration took place, as in the eating formica. Some days later the patients were tormented with pains in the arms, thighs and feet, accompanied by great pustules. Marcellus cured the disease by means of bleeding from the saphena, sometimes from the basilica ; digestives, purgatives and finally frictions in the necessary places. When not treated the pustules sometimes lasted a year or more, making the patients look as if they had leprosy or small-pox ! Simon thinks the pustules on the penis were small chancres beginning as miliary vesicles, a view strongly in accord with that of the first author on syphilis, Conrad Schellig, who describes the disease as beginning like a millet seed. But for my part I am quite at a loss to make out how the lineaments of any known disease are to be recognized in such a confused description. Pains in the limbs are not an accompaniment of itch, nor does this disease ever make people look as if they had the leprosy ; true miliary chancres are rare, whereas the affection seen by Cumanus seems to have been quite

¹ *Op. citat.*, p. 175.

common, and I need scarcely say that bad syphilis was never yet cured by such means as he speaks of, unless the frictions were mercurial and played the chief part. My decision therefore would be to dismiss the whole account as far too imperfect to admit of our placing reliance on it, and indeed the observations were only written on the margin of a copy of Argelata's Surgery.

Farther Decline in Severity.—The reasons for doubting an improvement in the character of syphilis soon after its outbreak having already been given, it only remains, in connexion with this part of the subject to notice a farther progress of the kind, which has been anticipated, or perhaps we might call the mental process by which it was evolved, semi-predicted. Mr. Lee ascribes the opinion spoken of to Swediaur, who, he says,¹ was satisfied not only that syphilis had grown milder after some time, but that this amelioration had progressed till there was a possibility of the disease subsiding, in happier ages, into a mere local affection. Mr. Lee does not give the part of the work from which he quotes, but he is known to be extremely careful. In the only edition of Swediaur² which I have consulted about this part of the matter, the direct opposite is stated. "I have," he says, "seen the disease in a number of instances as virulent and inveterate as ever described by any writer of the sixteenth and seventeenth century." He thought, however, that the complaint was not seen so often, and he was disposed to attribute this to the treatment being so much improved, though it might also be due to the poison having grown milder. But if Swediaur did not hold to the opinion first expressed, many others did so, in particular Astruc, who thought it was dying out in his time, or to use his own words³ saw "probable Grounds to hope, now daily approaches towards its Declension."

Swediaur is, I believe, at any rate held responsible for the statement often urged in support of the theory about the former severity of syphilis, that when this disease has appeared in a new country, it has always been in a malignant form, the violence of which lessened with time; his share in the opinion however seems to be founded on the history of the outburst of syphilis around

¹ *Syphilitic and Vacino Syphilitic Inoculation*, p. 153.

² The third; 1788.

³ *Op. citat.*, Preface, p. vii.

the Bay of St. Paul. Now it may seem wrong to attack what has become almost one of the pillars of syphilitic pathology, to question a saying which has circulated peaceably through two or three generations of authors ; but Swediaur does not prove any malignity beyond what is common to neglected syphilis, and he stands convicted of far too serious errors to allow of our receiving his word unsupported by much better facts than he adduces. He professes to have drawn his materials from the narrative of a Mr. Bowman, who investigated this disease on the spot ; he also mentions that government, in consequence of the humane representations of Governor Hamilton, sent out six surgeons to treat, and provide with medicine, gratuitously, every person suffering from this new disorder. But when Dr. Adams inquired he was told that no one of the name of Bowman had ever been there in any such capacity ; that there never had been any such person as Governor Hamilton, though Swediaur expressly speaks of him by that name,¹ nor could any minute or entry be found of medical men being sent out. The real names seem to have been M. Beaumont, of whom I find no farther mention, and Governor Haldiman ; an author, therefore, who was so lax on one head, might not have taken due pains with respect to another, and this is the reason why I expressed myself guardedly about believing the number of people said by him to be affected with the new disease.

Diminution in the Number of hard Sores.—The next ramification of the belief, that syphilis has abated in virulence, is the oft repeated story about our so rarely seeing the true hunterian chancre now-a-days, a belief of which Richard Carmichael seems to have been the author.² The opinion may have some seeming foundation ; there may be here and there a temporary change ; but as to giving this the validity of a law, as to representing the sore to be continually declining in numbers, which I suppose is what some later writers mean, I see nothing to support and something which confutes it. Neither Hunter nor any author of his day goes into the statistics of the question ; consequently there is no safe starting point to date from in comparing his time with Carmichael's. If the latter relied solely on his own experience, the fact shows how soon an occasional circumstance is mistaken

¹ *Op. citat.*, p. 176.

² *Op. citat.*, p. 60, 337.

for the operation of a law, and how easily a man of great abilities is borne away by a hurried judgement ; for a decline, so rapid as to make itself felt between 1786 and 1819, would ere this have ended in the almost total extinction of hard sore in Dublin, which is not the case.

Such a reason would, I submit, justify us in rejecting Carmichael's doctrine, but it may be as well for form's sake to go a little into statistics. Hunter then speaks¹ of syphilis occurring in the proportion of one case to four or five of gonorrhœa. From a number of cases noted by myself, I computed that simple sore is rather more prevalent than gonorrhœa. Now if we take the only figures which I know of issued near the time of Carmichael, namely those of Sir George Ballingall, we find, guessing as well as we can, that the proportion of syphilis to gonorrhœa was then most likely even a little higher, as he gives² the numbers in the Mediterranean fleet for 1835 at 595 of syphilis to 234 of gonorrhœa, and for 1836 at 710 to 282. Allowing that many of these cases were re-entries, instances of constitutional disease and so on, we must also deduct something in this way from the gonorrhœa cases. M. Rollet, a quarter of a century later, states³ that a scrutiny of above two thousand cases gave about five-twelfths gonorrhœa, four-twelfths simple, and three-twelfths infecting, sore ; so that I fancy we should scarcely err in assuming that seven cases of primary sore represent Hunter's four or five of gonorrhœa. This would show, as well as such rough calculations can be supposed to show anything, an average of one hunterian chancre to eight sores of all kinds.

An analysis of more recent information leads us to think that this proportion of hard sore is at least maintained in our day. Dr. Jeffery Marston gives⁴ the proportion of soft sores to hard as four to one. Mr. Peter Comrie puts down⁵ the number of infecting sores, which ought to be somewhat in excess of the true hunterian, as one to four or five of soft sore, though he says there is sometimes a run of the former. Mr. Sloggett noted in the

¹ *Treatise on the Venereal Disease* ; 1786, p. 217.

² *Outlines of Military Surgery* ; 1855, p. 506.

³ *Recherches sur la Syphilis* ; 1861, p. 29.

⁴ *Report of the Committee on Venereal Disease*, p. 21. ⁵ *Ibid.*, p. 89.

Edgar¹ a hundred and sixty-seven cases of soft chancre and sixty-seven of hard. Dr. Robert Beith gives² the relative frequency of hard sore to soft as one to three. Dr. John Morgan gives³ the admissions in Dublin, for three months, at twenty-one hard sores and eighty-eight soft, and those from the Curragh camp for the same time at seven hard and thirty-four soft. In private practice he thinks⁴ the hard sore is somewhat in the ascendant, a point on which I agree with him. M. Fournier found one case in three to be infecting,⁵ and M. Puche, in ten thousand cases of sore, met with nineteen hundred and fifty or almost one-fifth of hard chancre⁶ beyond which figures I think I need not go.

Of course it will be said that all hard and infecting sores do not come up to Hunter's type. The objection is no doubt strong, still such sores, discriminated by careful observers, contain inherently a large proportion marked by genuine hardening. Besides, as I understand Hunter, he in no way restricts the primary lesion of syphilis exclusively to the form which he portrays, and it was only natural that he should select the most characteristic variety for his famous description. Weighing all these points then, I think if we draw any inference, it must be that there is not decisive evidence of change at any period since Hunter's time.

Possible Changes in Syphilis and their Law.—But is syphilis, in place of undergoing any such mutations, widening and tightening its grasp on the human frame, and is this part of some great general change? There are good reasons for asking both questions. Venereal iritis seems to have been non-existent in the days of Hunter and Pearson; according to Lawrence⁷ it was unknown to German oculists till Schmidt described it, and Mr. Judd says⁸ he does not remember to have seen it when he began his medical studies. M. Chaballier maintains⁹ that the disease of the eye, mentioned by De Vigo and rendered by his old translator "ophthalmia," was nothing more or less than iritis, a view strongly combated by Fournier,¹⁰ the fact being that the point may with

¹ *Report of the Committee on Venereal Disease*, p. 130.

² *Ibid.*, p. 142.

³ *Op. citat.*, p. 17.

⁴ *Ibid.*, p. 24.

⁵ *De la Contagion Syphilitique*; 1860, p. 109.

⁶ *Ibid.*

⁷ *A Treatise on the Venereal Diseases of the Eye*; 1830, p. 3.

⁸ *Op. citat.*, p. 478.

⁹ *Op. citat.*, p. 85.

¹⁰ *Jean de Vigo*, p. 96.

equal probability be decided either way. But about the absence of iritis in the days of Hunter and Pearson I think there can be no doubt. These two great surgeons were justly famed for their powers of observation ; the one had a large pathological experience of the disease ; the other had perhaps the largest practice of his day in venereal complaints. To me it seems inexplicable that such an affection, especially if it were as common in their day as it is now, could have escaped the notice of the most inattentive, and for a still stronger reason that of men of such keen perception as the authors just named. One might think that the altered appearance of the eye would court detection, and even if the medical men overlooked so striking an affection, how came it that the patients never complained of the pain, discomfort and interference with vision as they do now? There is hardly anything a man dreads so much as the danger of becoming blind, and the encroachments of iritis are enough to alarm the most apathetic. In short, while I can understand that such an affection as syphilitic ulceration of the eyelid, the discovery of which is claimed by Lawrence,¹ may have passed unheeded, I admit my inability to believe that iritis ever did so.

For very similar reasons I feel driven to conclude that syphilitic fungus of the testicle is a new arrival. The rapidity with which almost the whole and in some cases the entire gland herniates through the opening, and the singular and to the patient alarming appearance which it presents, conspire to make an impression on the least reflecting mind. It appears to me impossible that such an affection could have existed without strongly attracting notice, yet it seems certain that, though pointed out I think by Carmichael,² it was practically unknown till Rollet described it.³ The constantly increasing number of contributions to the syphilitic pathology of the nervous system does not come to our assistance here, as we cannot decide how much of the increase is due merely to improved modes of investigation.

As concerns the question how far this is part of some great general change I must deal with it briefly, and I fear very imperfectly, as I have only space for a few scattered hints. It would be

¹ *Op. citat.*, p. 308.

² *Op. citat.*, p. 241, 3.

³ *Op. citat.*, p. xxi.

easy to get up a formidable array of testimony, but as just stated, my limits compel me to narrow the evidence to a very few points, and those chiefly connected with a disease often supposed by authors to have supplanted syphilis, namely leprosy, and an affection which has been considered, on no very satisfactory grounds, to be in some way connected with leprosy, that is to say, lepra.

Of these the first is the mysterious disappearance of leprosy itself. Respecting this it will be unnecessary to offer any proofs, and indeed I start with the theory that the reader is fully acquainted with the history of this part of the question. Neither will it be pertinent to the question to state when and where its decadence began. Suffice it to say that leprosy is gone, and that not one of the reasons, as yet assigned for its decline, will bear the test of critical examination. Unless, as I pointed out some years ago, we fall back upon the supposition that this disappearance is due to those unknown springs of action, possibly great climatic and terrestrial changes, which have swept off so many successive races of men and animals, there is no conceivable cause to account for the phenomenon. The next point is the equally mysterious disappearance of lepra for a time and its return with the present century. That this disease existed in far off ages scarcely admits of a doubt, several of the authors who describe leprosy speak of its symptoms in terms which could not well apply to any other complaint. From the time however when leprosy quitted the stage, lepra seems also to have vanished, and to the best of my knowledge it is not spoken of in clear and definite terms by any writer of the seventeenth century. Beckett seems clearly to have recognized it, mistaking it however for leprosy, and Hensler's wonderful industry yielded him a stray case or two culled from the writings of Mead, Brisbane, Fischer and Vogt;¹ a treasure trove which seems to have quite gladdened the heart of the fine old scholar, for he speaks of traces of lepra being found in Europe, as he might do if a hitherto unknown race of men had been discovered. But a physician so near our day as Heberden never treated a case and Cullen never saw one;² statements cal-

¹ *Vom abendlaendischen Aussatze*; S. 342.

² Bateman; *Op. citat.*, p. 28.

culated to astonish those who know, that in the present day lepra absorbs quite five and a-half per cent. of all cases of cutaneous disease. Lastly we find Adams pointing out¹ the distinction between this disease and elephantiasis Græcorum with a persistence, which, of itself, goes far to show how little was then known of lepra, so common in our day.

Syphilis not derived from or transmuted into any other Disease.—Simon states,² as if it were a fact established beyond dispute, that in many parts syphilis, itself the offspring of leprosy, has degenerated into a variety of the latter disease. The reader is, I presume, quite aware that some of the older writers believed in such a transformation; others, Paracelsus among the number, thought the morbus gallicus was a cross between the early and local venereal disease, cambucca, and leprosy, an opinion not very actively assailed by M. Ricord when alluding to it,³ or even by a more critical author in matters of history, Simon; and Dr. Mason Good describes⁴ a form of leprosy, the Rose of Asturia, which might be mistaken in some of its features for syphilis, while Hensler, treading apparently in the very steps of Paracelsus, says⁵ that leprosy has become blended with other diseases. Thus we see that the idea of fusion, first, I believe, put forward by the daring but highly-gifted swiss innovator in the above shape, and when he maintained that serpigo is a mule begotten by crossing leprosy with syphilis, has survived its eccentric founder by more than three centuries, and has not gone entirely out of fashion now. As I am not aware that these opinions have anywhere been critically examined, I purpose to end this paper by briefly summing up the evidence for and against the probabilities of a change, which seems to me in its way of as much magnitude, and as difficult to realize in conception, as a transmutation of species. Besides leprosy, there are some other diseases into which syphilis is said to have degenerated, and it will therefore perhaps be most convenient to take them altogether.

Leprosy.—An analysis of the report on leprosy, issued by the Royal College of Physicians, disposes of the view that there is

¹ *Op. citat.*, p. 278.

² *Op. citat.*, p. 5.

³ *Op. citat.*, p. 162.

⁴ *Op. citat.*, Vol. 3, p. 43.

⁵ *Vom abendlaendischen Aussatze*; S. 2.

any fundamental connexion between this disease and syphilis. Whatever affinity, whatever similarity there may be, there is assuredly no tie between them in the way of descent ; nothing to show that leprosy ever gives birth to syphilis or any other disease, or that syphilis ever degenerates into it. The two diseases may run their course in the same individual ; a patient suffering from leprosy may contract either local or constitutional syphilis,¹ and be cured of either or both without the leprosy being affected. Leprosy is as nearly incurable as a disease, to be curable at all, can well be. No system of treatment can be relied on to influence it in the slightest degree ; syphilis can often be cured and almost always relieved, while in contra-distinction to leprosy it is rarely fatal. An infant is scarcely ever born with leprosy ; still-births from syphilis are common enough. The whole list of diseases therefore, given by Simon² as having undergone this suspicious degeneration, may be struck out. The phenomena, when analyzed, resolve themselves into leprosy or syphilis, or both ; the former disease perhaps dying out, the latter having possibly undergone much the same change as in sibbens. Of such affections the history usually is that they have become localized in some out of the way place, among people far removed from their only chance of help, the aid of surgery, and that these people, judging only from what they could see and feel, have given the malady some local name, describing its most prominent symptom. I purpose closing this paper with a few brief illustrations of the above statements.

Scherlievo or *Skerljevo* is one of the diseases thus long mistaken. The symptoms are aching pains, hoarseness, difficulty of swallowing, inflammation and unhealthy ulceration of the velum, fauces, uvula, and tonsils, the ulcers being covered with yellow lardaceous secretion. Having passed through this stage the disease subsides, and on its reappearance attacks either the osseous structures or the skin. Squamous, moist and ulcerative eruptions, and copper-

¹ Danielssen. Quoted by Lee ; *Syphilitic and Vaccino-Syphilitic Inoculation*, p. 50.

² “ Das Pellagra, die Sibbens, die Yaws, die Pians, die canadische Seuche, die krimmische Krankheit, das mal rouge de Cayenne, die norwedische Radsyge, die holsteinische Krankheit, die asturische Rose u. s. w. sind mehr oder weniger boesartige Formen der in Aussatz ausgearteten Lustseuche.”

coloured spots about the size of lentils, accompany these symptoms. The eye, nose and cheek are sometimes destroyed by ulceration. "Growths on the lips," tubercles, condylomata, nodes and fungoid ulcers, ozæna, contractions of the limbs, and "fungus of the joints" are mentioned among the symptoms. The disease is highly contagious, being communicated by contact. We find the hideous crusts from pustules, described by one or two of the earliest writers on syphilis, reappearing on the outbreak of this disease in Illyria and Dalmatia at the beginning of the present century.¹ Després suggests² that this disease is modified by scrofula, an opinion I cannot share. The addition of the strumous element seems to me purely gratuitous, and I see nothing in the description beyond what belongs to neglected syphilis, although it has been asserted that scherlievo is nothing more than leprosy, and in the absence of observations made on the spot, a certain degree of doubt may impend over some of the histories. Communication by contact, however, is certainly not a symptom of leprosy.

Sibbens, or *Sivvens*, is another. This disease, which seems to have been first systematically noticed rather more than a century ago in Dumfriesshire, is reputed to have been introduced thither from the Highlands, where it had long been known under the name now universally given to it. In a pamphlet³ now very scarce, for a knowledge of which I am indebted to the courtesy of the late Dr. Blacklock of Dumfries, it is stated that the disease began with a sore throat or an inflammation of the uvula, "pap of the hawse" being the quaint old term given to it. The tonsils were often superficially ulcerated, and frequently white specks and sloughs appeared on the roof of the mouth and insides of the cheeks and lips. A very small excrescence, or "fleshy sprouting like a rasp," often sprang up at the "corners of the mouth," and was considered a pretty sure sign of the disease. Sometimes there was hoarseness and the uvula was destroyed. Children at the breast affected with this complaint "perished for hunger," not being able to suck. In some instances the submaxillary glands were swollen.

¹ Haeser; *Op. citat.*, Vol. 2, p. 293.

² *Op. citat.*, p. 333.

³ *An Account of a Very Infectious Distemper*; 1769. No Name. Almost certainly by Dr. Ebenezer Gilchrist.

In a more developed stage the disease showed itself in the shape of small pustules, or “blushes of a dirty hue,” which broke, left a dry crust “with blueness around,” and ulcerated deep into the underlying cellular tissue or fat. This symptom was chiefly seen in children. The pustules mostly occupied the belly, groins and sides. They were sluggish, not large, often nearly round in shape, and with a clean, slightly inflamed edge. Occasionally they ran together, sometimes to such an extent that “all the fatty membrane of the belly below the navel” was laid into one huge ulcer, which emitted such an “intolerable and peculiar stench, that those in this condition might be said to be rotten before they were dead,” reminding us of the stories told about some of the earliest victims of syphilis. In some children the whole scalp fell into “a mortified state, the ears ready to drop off.” Smaller, very sluggish ulcers, which “always remained in a dead state, without pain or inflammation,” were likewise observed.

In a still more malignant state it seems to have been accompanied by an outbreak of impetigo rodens, for we are told that now boils of a “high florid colour, without any matter to defend them,” formed “in different parts, in the arms, shoulders, face, legs and feet,” where they degenerated into ulcers, penetrating to the muscles and leaving them bare. They were so excessively tender that they would not support the mildest application, and the lips of the openings so formed were hard and ragged. In some rare cases the disease affected the bones, but never “with us” the large and more solid ones. Benjamin Bell however, commenting upon this statement, says¹ that he had seen several instances where both the bones of the arms and legs had been attacked, and that it was “by no means uncommon to find this disease fix upon the bones of the head.” Several persons lost their teeth, with the sockets, and parts of the osseous structures of the cheeks and nose gave way in others. When the skin was alone or principally affected, copper-coloured mottling was the predominant sign. Children seem to have suffered from erythema on the lower part of the belly, buttocks, thighs, and parts of the legs, occasionally taking on the ring form which has been noticed by some observers. Broad red patches, as large as the palm of

¹ *Treatise on Gonorrhœa Virulenta* ; 1793, Vol. 2, p. 444.

the hand, were seen in somewhat older subjects, scattered over the trunks and limbs, and accompanied by inflammation. Or clusters of pustules (Qy papules) came out, followed by dryness and peeling of the skin, which was left tender underneath. Scabby eruptions were often seen on the scalp, forehead, insides of the thighs and groins; also small indurated papulæ which occasioned great itching. Inflammation, soreness and excrescences about the anus were frequent. Serpiginous ulcerations (eating tetter) also prevailed, and syphilitic ecthyma, accompanied by great heat and swelling. The resemblance of this to variola, as in the cases I have already alluded to,¹ is expressly stated in the old pamphlet. "One had them," it says² speaking of the tubercles, "spread thick over the whole body with matter as in the confluent small-pox, and died when the swelling began to subside." Papulo-squamous eruption (syphilitic lepra) and sluggish tubercles running into each other are clearly described. The disease was intensely contagious.

I suppose few will now contest the opinion that this disease was really syphilis communicated purely in the secondary form, and that the view taken of it by Gilchrist more than a century ago, namely that it was venereal, is the correct one. The picture drawn of sibbens by Benjamin Bell, a quarter of a century later than Gilchrist, is simply that of venereal disease, and has been pronounced by an accomplished french writer to be, allowing for the absence of all mention of chancre, a magnificent description of syphilis. Bell himself never entertained a doubt that it was of this nature, and Mr. Wills, speaking of the disease, of which he gives a very good account, as it appeared in his day in Ayrshire, Galloway and Dumfries, says³ it began with a condyloma or tubercle, and not, as most writers would have it, with a pustule. The disease was a frequent cause of abortion, although according to Bell⁴, "in some instances children are born with it at the full time and in a few it breaks out in the course of the first month after delivery." The disease was cured with mercury, and not unfrequently required a full course of it. Sarsaparilla, decoc-

¹ Page 52.

² Page 10.

³ *London and Edinburgh Monthly Journal*; 1844, Vol. 4, p. 283.

⁴ *Op. citat.*, Vol. 2, p. 446.

tion of the woods and burdock were useful, particularly in the "higher degrees of the distemper," and later experience showed the value of iodide of potassium in this stage. The prevailing impression was, as has often been reported of syphilis, that a person, who had once contracted the disease, never caught it again.

Yet men well able to judge have doubted whether this disease was syphilis. Dr. Adams, who went to Scotland on purpose to have a look at such a mysterious complaint, describes an affection which we can scarcely recognize in the pages of Bell and Gilchrist. He found wasting rather than ulceration of the tonsils, loss of the uvula, ulceration of the velum, glueing together of these parts by viscid mucus, and pustular eruption, all preceded by fever; "pustular appearances," cicatrices, scabs &c. The disease scarcely ever appeared as a primary affection of the genitals. He concluded that sibbens is a separate disease from syphilis, and his opinion has been shared by Hunter, Mathias¹, and Mason Good²; while Swediaur thought³ it was a cross between "the itch and the lues," and then referred it "under the syphilis."⁴ Even Mr. Wills, who seems to have so carefully studied the complaint, arrives at a decision little calculated to satisfy the exacting demands of modern criticism; for he pronounces⁵ sibbens to be quite distinct from true venereal but identical with venereal condyloma. Dr. Adams even went so far as to recognize⁶ in sibbens, not the venereal disease of his day but that of a time anterior to 1494, a view which, after a careful examination of the authorities he quotes, I feel myself unable to confirm.

The fact then that these men—Swediaur, who was in the way of getting at the truth; Adams, who was on the spot and so ably assisted in his inquiries by the leading medical practitioners of the neighbourhood; and Mr. Wills, who took so much pains with his subject, should all have been mistaken, shows how easily such an error might have happened in the infancy of syphilis when every man had to be his own teacher. If more than two hundred and fifty years after the dependence of secondary disease upon chancre,

¹ *The Mercurial Disease*; 1816, p. xi.

³ *Op. citat.*, p. 12.

⁵ *Op. citat.*, p. 286.

² *Op. citat.*, Vol. 3, p. 417, 419.

⁴ *Ibid.*, p. 236.

⁶ *Op. citat.*, p. 190.

and the distinctly contagious nature of the former, had been laid open to the eyes of all thinking men, we find some of those who saw sibbens in its nestling place, so to speak, unable to satisfy themselves as to what it really was, and evidently disbelieving the infectious nature of secondary syphilis, we can easily understand that such oversights were possible enough on the part of those who saw the very first stray cases of this disease ; and that the absence of an authentic account of the importation of syphilis into a district is something widely different from proof, that the disease itself did not exist in that part of the world prior to the mention of it in history. That syphilis was really carried to Dumfriesshire from the Highlands seems fully established, and that it had infested the north-west of Scotland quite a century before the first account of it was given to the world, equally so ; but as to how it got into the Highlands, we have nothing beyond a tradition, which may be quite correct, but which is, as we might expect, entirely unsupported by authentic testimony. Consequently I see, in this fact alone, a certain degree of evidence, that the conjecture thrown out respecting the possibility of syphilis having lurked in Europe, for some time before its great outbreak, is anything but improbable.

I think the discrepancy between the opinions taken up by Adams and Wills and those held by Gilchrist and Bell admits of easy explanation. Syphilis thus communicated often runs a much milder course than ordinary venereal disease. This was peculiarly so with one of the patients seen by Adams¹, a young woman whose case he watched with great care and who recovered in a few days under mercury ; a fact the casual occurrence of which is vouched for in Dr. Gilchrist's pamphlet, though evidently enough by no means the rule, for it is stated that for the most part the distemper returned, perhaps in a worse form, and was then only to be eradicated by a regular course of medicine like that recommended in venereal cases ; that is to say mercury, for the employment of which he gives singularly judicious and succinct directions. Mr. Wills also observes that the secondary manifestations of sibbens are mild, and so far as his observations went, with one exception, wholly confined to the skin, he having in a practice of

¹ *Op. citat.*, p. 184.

twenty-seven years seen nothing of destruction of the bones ; adding, however, that though mercury internally and caustic to the condylomata generally cure quickly enough, the disease often returns and demands a sharper and more prolonged treatment. Still more misleading was the absence of chancre and the regularity with which, after an incubation of about a month, the disease broke out in the throat, tonsils and mouth.

So far all is intelligible enough, but we now come upon a source of mystery and error, which seems to be difficult enough to deal with, and this is the presence on the scene of the yaw fungus, the very thing from which sibbens takes its name. This symptom was a "spongy substance" which sprouted up "much like a rasp or a strawberry, elevated one half above the surface, and, when fully formed," appearing "as if set in a socket cut exactly in the flesh to receive it." Dr. Gilchrist further describes the fungus as springing up in a patch of pityriasis, "an itchy tetter or ringworm" of a round form, which, either spontaneously or from scratching, gradually came to discharge an ichorous humour. The tetter itself was sometimes crusted over with a black scab, except at the edges where a crack or ring formed, "like the line of separation between a mortified and a sound part." By degrees this crack enlarged, the scab was "pushed off" and was succeeded by the fungus. At other times the tetter was represented by "a dark or grey scarf resembling some kind of leprosy." The fungus seems to have arisen most frequently on the tetter which did not become crusted at all, and is characterized as being indolent rather than tender. According to another observer the surface of the fungus was covered with a secretion like toasted cheese. Of the descriptions by Hill, Trotter and Freer I am unable to give any account, as their writings have either perished, or so entirely disappeared from circulation, that I have not met with them anywhere.

Dr. Adams tells us¹ that this fungus was not insisted upon as a necessary symptom of sibbens by some of the best practitioners whom he consulted. The reason is not far to seek ; they had never seen it. Dr. Gilchrist, in the pamphlet, expressly says² that it had never been observed in Dumfries, nor is there anything in

¹ *Op. citat.*, p. 193.

² Page 11.

Bell's description showing that he ever saw it. Neither do I find better proof of personal observation in the writings of Hibbert and Wills. A symptom so prominent as this could not have been overlooked, and as it is not the nature of syphilis to appear, under two different shapes, in places so near as the Highlands and Dumfries, I am forced to conclude, either that yaws, to which this fungus pretty clearly belongs, existed in the Highlands, or that Gilchrist and others drew their materials from oral accounts, confused by passing from one person to another.

Each of these conclusions seems equally difficult to sustain. An occasional case of yaws may have been carried from a tropical climate to the Highlands, but all the evidence, which I have been able to collect, is opposed to the view that such a disease would maintain itself and be propagated there. Dr. Hibbert indeed contends¹ that it could and did; that yaws was only sibbens intensified and masked by the dirt, stench, wretched food and utter want of ventilation then prevalent in the Highlands and western islands of Scotland, and in no way different from the tropical disease, the characteristic features of which are simply the product of misery. This view was, in reference to identity, long previously opposed by Dr. Gilchrist, and later on evidence will be brought forward to show that he was most probably right. It was however again upheld after his time in a "Medical Sketch of Dumfriesshire," by Mr. Gibson, one of the surgeons to the Dumfries Dispensary; and Dr. McCulloch's opinion is that sibbens is neither more nor less than frambæsia, and that it was brought into Dumfriesshire by a company of soldiers from the West Indies who were quartered in that town. The reader will see, in the section on yaws, that this opinion is contested by a gentleman now residing in a part where yaws is very frequently seen, and the statement of Gilchrist about the entire absence of yaw fungus in his neighbourhood is yet to be explained. It may however be, that frambæsia degenerates in a cold or temperate climate, and that both it and syphilis were separately introduced into both the Highlands and Dumfries.

That any syphilitic growth ever really assumes the form and look of a raspberry or strawberry, or is ever covered with a

¹ *Edinburgh Journal of Medical Science*; 1826, Vol. I, p. 309.

secretion like toasted cheese, I must beg leave to doubt. I have seen a good deal of neglected syphilis, but nothing which could, except by a strange abuse of language, be so described. Some years ago I had under me a lad from Barking, evidently of rather obtuse intellect, and suffering from secondary disease which had been left to take care of itself. He had, on the right side of the scalp and forehead, two condylomata of such enormous size, that I tried, in vain however, to induce him to let me have a drawing made of them. In their way they were by far the most revolting and preternatural things I ever saw, each of them being at least six times as large as a good sized condyloma; but as to comparing them even in the loosest way to the formation and appearance of the rasp- or strawberry, the simile would have been very far fetched; and the secretion, with which great part of their upper surface was overlaid, reminded me of what I should think dirty, thin, white kid leather would be when boiled to a pulp. Perhaps the less imaginative language employed by Carmichael in describing button scurvy, which seems to be the same disease as sibbens, may offer a clue to the solution of some part of the mystery, for he speaks¹ of the secretion as being a "white tenacious matter," and of the spots as "exhibiting an appearance somewhat like the surface of a raspberry." Mr. Wills says² it was essentially the condylomata in the groins which, after the delicate cuticle had peeled off, took on "somewhat the look of a raspberry in miniature," which may have been strictly correct, but does not quite explain what Dr. Gilchrist says when describing the fungus, any more than it does why the common people in Dumfriesshire and Galloway used to call sibbens by the name of yaws,³ a thing they would hardly have done, unless there had been either occasionally a stray case of yaws itself, or else one of sibbens more strongly resembling it than is described as having been seen by any of the writers I have quoted.

Yaws is the last of these affections for which I can find space, and I suppose any one who described the pathology of it as being in a state of well-nigh hopeless confusion would not be far wrong. It will, I trust, not be held requisite, that all the evidence in proof

¹ *Op. citat.*, p. 16.

² *Op. citat.*, p. 284.

³ B. Bell; *Op. citat.*, Vol. 2, p. 437.

of this should be cited, but a few specimens, showing the wide discrepancy of opinion, not only as to the nature but even as to the symptoms and course of the disease, are indispensable; those selected are taken almost entirely from the writings of the later authors who had themselves seen and studied frambæsia in its native haunts.

According, then, to Dr. Adams, who had an unusually good chance of watching the progress of a case, the disease begins with intermittent fever; after the "remission" of this a universal eruption of pustules takes place, with sore throat, and small ulcers on glans penis. In the case which he narrates fifty-six of the largest pustules, some of them not less than two to three inches in diameter, had ulcerated at the end of a month, when the sore throat still continued and the fever was as violent as ever. In addition to the large pustules there were numerous smaller ones. The scabs which formed on the pustules were horny, and on removing them a fungus shot up. The disease appeared to die out by a process of exhaustion as in smallpox, the whole constitution being affected at one time and gradually throwing off the incubus. Mercury seemed to exert little control over the affection. Adams confirms the statement of Ludford, that the mother yaw, a large tubercle in which the severity of the disease seems to culminate, always leaves a scar. I suppose this means that the others were not followed by citatrices, as would certainly have happened had the case been one of syphilitic ecthyma, to which otherwise, and excepting also the fungus, it bears a strong resemblance. Adams, however, and Hunter looked upon yaws as a distinct affection, as did Swediaur¹ and Mathias; facts to be remembered to their credit, when we consider how authoritatively it has been stated that yaws is only neglected syphilis, an opinion in which I quite admit having shared up to the time when Dr. Gavin Milroy's report appeared, and which is still in so far shared by Kaposi, that he maintains² the idea of yaws being a disease sui generis must be given up, as must equally the opinion that it is always syphilis, he having shown that the growths, which distinguish it, are also found in non-syphilitic, inflammatory, and neo-plastic processes.

¹ *Op. citat.*, p. 239.

² *Die Syphilis der Haut*; 1875, S. 155.

Dr. James Thomson, who had seen the disease in Jamaica, describes it again very differently from Adams. According to his account, which I take from Dr. Hibbert, not having been able to find the original where he gives it, the disease is frequently ushered in by feverish symptoms, and the whole skin appears as if dusted with flour. The readers of Hensler will recognize this symptom as indicative of leprosy, while, according to Wendt,¹ who however does not impress one very highly as to his learning or discrimination, it constituted an element in the morbus gallicus. After the lapse of a few days, small papulæ appear on the forehead and other parts of the body, continuing to increase till a crust forms on the top, under which ill-conditioned pus is found. A pustule, thus covered with a scab, will often increase to the size of a shilling, concealing a foul sloughy ulcer, which usually heals, though sometimes a fungus springs up in it.

Dr. Pedrelli is somewhat inclined to believe² that there are two kinds of yaws, but he does not speak positively. One kind is confined to a particular locality, principally the torrid zone, among the natives of which it is found; it is of a virulent nature and due to causes not yet ascertained. The other is not restricted to any particular race or region, and is possibly of syphilitic origin. Dr. Pedrelli has, perhaps, with some slight reservation, come nearer the mark than any other writer; for while there can be little doubt that misunderstood syphilis, communicated by secondary affections, and identical with sibbens, has sometimes been mistaken for yaws, there is equally little that, as expressly stated by some of the respondents in Dr. Gavin Milroy's report, a distinct endemic affection of that name exists, "a specific disease in no way allied to syphilis or leprosy,"³ which will be described farther on. The reservation just made is based on the two following points. It does not really appear that sibbens is found in the tropical regions where true yaws is endemic, or that the latter is more virulent and obstinate than neglected syphilis. Some of the

¹ Hufeland's *Journal der practischen Arzneykunde*; B. LV, S. 4.

² *Bulletino delle Science Medicales*; Ottob., 1871. Quoted by Dr. Purdon; *Cutaneous Medicine*; 1875, p. 260.

³ *Report on Leprosy and Yaws in the West Indies*, p. 57.

cases at any rate are mild enough. Dr. George Turner, who saw the disease in Samoa, found¹ that mercury had a great control over the severity of yaws, a few doses causing the pustules to shrivel up and disappear, a statement which recalls what has been often said of sibbens itself, and Dr. Turner's account is quite substantiated by some of the replies in the official report on yaws. The distinctive feature here is that sibbens, under which is comprized everything in the shape of the syphilitic form of yaws, sometimes manifests a very intractable disposition, and may under certain circumstances develop into ordinary syphilis, which, as well as I can judge, is far more formidable than yaws proper is described to be.

According then to what is said in the report, the latter disease is inoculable, and when the virus is inserted, the sore "may heal up without any appreciable change before any constitutional symptoms arise," or it may turn into a "roundish ulcer," "with everted edges and depressed centre." The latent stage, that of incubation, lasts "about four to six weeks, after which there is an attack of feverishness, with pains about the joints and shafts of the long bones." After this follows a roseola or pityriasis, as also constitutional irritation accompanied by "severe aching pains in the bones and joints." Then "come small, flat spots, or blotches of a brownish or dark-red coloured efflorescence." On these form tubercles, covered with cuticle and of a light yellow or copper colour. Out of or on these tubercles the yaw proper arises, a round or "dark convex" growth very like "a raspberry, strawberry or mulberry." There may be ulcers on the feet or legs, "nodes and swellings of the joints" and diffuse inflammation of skin of palms of hands and soles of feet, followed by escape of sero-purulent fluid, giving the part "a riddled or sieve-like look."

The disease spreads by contact, never to all appearance springing up spontaneously, though one observer tells a different tale, basing his judgement however seemingly on only two cases, one of which is suspicious, the patient having had syphilis. Mercury assists "in the dispersion of yaws" and iodide of potassium is useful especially when the bone pains are bad. One of the medical men speaks of mercury as the sheet-anchor, and

¹ *Glasgow Medical Journal*; New Series, Vol. 2, p. 509.

praises black wash. "The general impression is that, after one attack of yaws, the person becomes insusceptible to the contagion of the disease," a position contested by another observer. Dr. Milroy enumerates the symptoms in a case of long standing as "numerous scabby spots over the trunk and extremities of the size of small or largish limpets, and covered with dry yellow crusts, the removal of which exposed a raw surface sometimes bleeding a little and having a red granulated or slightly fungoid appearance, but with little if any purulent discharge on this surface." Another patient, seen by him, had large superficial ulcerations on the front of the ankle and dorsum of the foot, and so on. The "scabby spots," the size of limpets, covered with crusts, recall to mind the accounts given of the malmorto and first cases of syphilis.

According to the foregoing descriptions, the disease is not a bad imitation of syphilis communicated by contact, and we might really feel inclined to adopt the belief that yaws is syphilis in the African, its salient features somewhat blurred and even effaced by such modifying influences as neglect and difference of constitution. But now let us hear the account given by another medical authority, whose narrative differs widely from that of some of the respondents; it is that of Dr. John Imray¹ and is to the following effect.

He says the ordinary yaw excrescence does not resemble a strawberry at all. It is "not unlike a piece of coarse cotton wick, a quarter of an inch, more or less, in diameter, and stuck on the skin in a dirty brownish setting, and projecting to a greater or less extent." These growths generally appear on the face, neck, extremities, parts of generation, perinæum, hips and arms, much less frequently on trunk and hairy scalp. May form on nostrils or on eyebrows, becoming then pendulous; also about mouth or anus, constituting almost a ring round the opening. They leave in the negro marks darker than the skin, but in a person with a mixture of white blood these are whiter than the natural hue. "It is rare indeed that the throat, palate or nasal bones become affected." There is usually "very little if any constitutional disturbance, either during the period of incubation or on the accession of the

¹ *Report*; p. 12.

eruption." If one member of a family contract the disease, all then susceptible of the infection take it. Yaws, he considers, is communicated only by contact, and a period of seven to ten weeks elapses between the inoculation of the virus and the breaking out of the eruption. The affection is almost exclusively confined to the blacks, but cases do occur among the whites. With care it can almost always be cured.

The only forms of constitutional syphilis, he contends, which resemble yaws, are tubercles and condylomata ; but the tubercles of syphilis are solid and deep seated, while those of yaws possess neither of these characters ; farther, the latter are not prone to ulcerate, and they begin as small yellow spots, which gradually grow more prominent till the cuticle gives way and a spongy mass appears. Even when they assume the appearance of condylomata, as on the anus, the characteristic yaw fungus will be found on other parts of the body. He has never heard of a case which followed a primary sore (chancre). Calomel or corrosive sublimate in decoction of the woods, with great attention to cleanliness, seems an effectual remedy, a feature in which this disease, or variety of disease, again shows a resemblance to sibbens, and contrasts strongly with the rebellious nature of true syphilis communicated by chancre.

Towards the close of this description, unsurpassed for completeness and lucidity, Dr. Imray sums up adversely to the supposition that yaws is identical with sibbens. The reasons are that the latter begins with ulcerations on the tonsils and uvula, and aphthous ulcerations on the inside of the mouth and cheeks, while frambæsia never commences in this way. Dr. Milroy pronounces no opinion on the question of identity. Taking, then, Dr. Imray's account as strictly correct, we must conclude that Adams, Mathias and those other authors who considered yaws to be a distinct affection, were right ; that there is, properly speaking, only one disease of that name ; that *yaws, though not venereal, is a syphilis*, having more analogy with that disease than any other ; and that, after Dr. Milroy's researches, to call any form of secondary disease yaws, means not pointing out a variety of disease but committing a mistake, which, prior to the publication of the report, still seems to me, so far as I can judge, difficult to

avoidance. Dr. Hibbert sees in the syphilis of the close of the fifteenth century nothing but yaws, the product of misery engendered by the long devastating wars and breaking up of the feudal system &c. He would have come nearer the mark had he found it in the description of the deed evyll (malmorto) by John de Vigo, with its pustules raised above the level of the skin, and their colour that of a mulberry half ripe. As it is, I do not understand how he arrives at such a conclusion, for there is not, in any one of the authors whom he quotes, a single passage which could stand for a picture of the yaw fungus.

Diseases resolved into Syphilis and Leprosy.—Several other diseases of a similar nature are mentioned by authors, the pians, the mal de Cayenne, the Rose of Asturia &c. I have already given a list of these, so that it will not be necessary to go through this part of the matter again. The early history of them is obscure, and even the most recent accounts are not very clear. Some are leprosy, but as in the case of the asturian Rose described by Dr. Mason Good, with features so modified as to suggest a suspicion that those who first described the disease did so more from hearsay than actual observation, or were biassed by what they had read in the older writers, and that the narrative had not improved in accuracy by passing from hand to hand. In certain cases, *e.g.* the asturian Rose itself, there is really good reason to think that a non specific, obstinate, misunderstood and neglected erythema was mistaken for leprosy and constituted the disease; in others we find secondary syphilis mixed up in the description with a separate disease, as in the canadian “mal rouge de la Baye de Saint Paul,” which seems to have included lupus exedens.¹

Some again are dying out. The late Dr. Blacklock of Dumfries was kind enough to inform me that in a practice of forty-one years he had never met with a case answering the description given by Dr. Gilchrist; and Dr. Borthwick, also of Dumfries, wrote to me that he had never seen the disease in the virulent form described by Dr. Gilchrist and others. Dr. Gilchrist, however, of the Crichton Institute, to whom I am indebted for some most valuable information tells me, that though Dr. McCulloch,

¹ Swediaur; *Op. citat.*, p. 172.

80 *Diseases resolved into Syphilis and Leprosy.*

the oldest practitioner in Dumfries, has not seen a case of sibbens for years, yet that Dr. Murray of that town has at present (May 31, '79) a case under his care. The decline of sibbens in Dumfries seems however to have been quite a recent affair. Dr. Gilchrist has been so obliging as to send me the entries of cases of this disease treated in the Infirmary of that town from 1789 to 1823, both inclusive, or a period of thirty-five years. The numbers exhibit singular fluctuations, but nothing like a falling off in the supply. Thus while the years 1802 and from 1804 to 9 display a blank, and 1798 and 1801 only yield one case each, the number in 1789, eight, was nearly quadrupled in 1794, and after being several times at such figures as twelve and fifteen, closed in 1823 at nineteen. Four years after we find Mr. Gibson speaking of the disease as if it were rare. "Frambæsia or yaws," he says, "is occasionally met with, but seems to be becoming less frequent. Regarding sibbens my correspondents have mostly been silent." Allowing a good deal for increase of population, and the greater facilities afforded to those suffering under these diseases for having their cases attended to, there is yet something to be explained about this long persistence and sudden disappearance of sibbens. Again as to yaws. Dr. Daniel, the african traveller, told me that in a residence of sixteen years on the west coast of Africa, where this disease was once supposed to be so common, he had not seen more than six or seven cases of it, and these he had not examined sufficiently to satisfy himself as to the real nature of the disease, and surgeons have informed me that yaws is now rarely seen in the West Indies. This statement is confirmed in the report of the Royal College of Physicians on leprosy¹ as regards Jamaica, and in Dr. Gavin Milroy's report² as regards some portions of these colonies, Barbadoes and Antigua for instance, but rather confuted as to others, among them the country districts of Jamaica, where whole colonies of Africans are said to be "relapsing into all the barbarisms of their native condition,"³ and Dominica.

Dim, however, as is the light shed upon these strange complaints, it yet reveals one thing; no transition between syphilis and leprosy or any other disease, no hybrid affection which could

¹ Page 10.

² Page 72, 51.

³ Ibid., p. 62.

be referred to any possible conjunction of them, has yet been found. As the nebula of one age is resolved by the higher telescopic powers of the next into stars and space, so does the confused picture here, when the beam of critical investigation, gathering strength of light with increase of ages, is turned upon it, settle down into the two forms of disease just named, their natures, even in the long lapse of years, as unchanged as those of the elements. When we come face to face with the symptoms, we find that the phantom maladies described by Simon and others have no real existence; they die out and the twin giants, syphilis and leprosy, reign in their stead.

Transmutation of Syphilis into Cancer.—But wherever we turn some wonderful transmutation of syphilis meets us, and we might almost say of faith in such points as Pallas-Athena says of her beloved Ulysses, that it “is not yet dead upon earth.” What is more, it seems bent upon dying hard, and if beaten from its former strongholds, a creed like this can easily, and indeed does take shelter in the refuge which cancer and scrofula offer, there being however this consolation for its pursuers, that if followed up it may be hunted out again with equal success. For there is no proof, pathological or histological, that any such change ever takes place. The theories about the transmutation of syphilis, and the facts on which they are built up, the fabric and the foundation, prove, when scrutinized, alike intangible. Cancer may spring up on a site previously or actually syphilitic, just as it may in a tissue injured by a blow; deterioration of tissue may excite increased activity in a cancerous disposition of the part; but as to such an absolute and radical change as is implied by transmutation taking place spontaneously, I must, on the evidence before me, class it as mere conjecture. Dr. Bradley clearly leans to the belief that syphilis may be transformed into cancer and struma, having met with an encephaloid tumour which sprang from a typical syphilitic patch on the retina,¹ and his views are I believe shared by men of great experience, but I cannot find out that they are in any degree proven. Again, while I have just admitted that the deterioration of tissue caused by syphilis may evoke cancer, I know of no single fact which really shows that it

¹ *Medical Press and Circular*; 1877, Vol. 2, p. 81.

has ever done so, though I have heard the doctrine maintained by an able surgeon. In deciding such a point identity of site must be left out of the question. Epithelioma has been known to spring up in the middle of a patch of lupus, and fungus hæmatodes in a mole, but this is something widely different from the change of one disease into another.

Equally little foundation do I see for the theory that epithelioma may be engendered by previous syphilis. M. Després, who has remarked nothing confirmatory of M. Ricord's doctrine that syphilis leads to cancer, has twice observed epithelioma "on the glans and uterine neck" in persons who had confirmed syphilis. Nothing more probable when we reflect that one disease is common, and the other not so very rare; but when we also reflect that epithelioma assails thousands who never had syphilis, and syphilis tens of thousands who never have epithelioma, it becomes very likely that such cases as these mentioned by Després are mere coincidences, the syphilis being an accident happening to persons who would under any circumstances have suffered from epithelioma, provided only that they lived long enough to give it a chance of showing itself. Kaposi's view¹ that syphilis acts here as a general not a special factor, is the only one to which a judgment, unbiassed by preconceived views, can conduct us; and admitting that it operates at all, I must contend that its range is very limited, for did such changes occur frequently, I must almost certainly have now and then lighted upon something of the kind in the large number of cases of syphilis in all stages which I have seen.

Into Scrofula.—I suppose there is no one who has read medicine at all, and yet has not read that syphilis, after it has percolated through the tertiary stage, may reappear in the children as scrofula. That it may be an occasional factor in the genesis of this disease, that it may act generally as an exhausting agent in the parent, an indulgent critic will perhaps class among the possibilities of syphilitic pathology, but I know of nothing which attests the probability of such a surmise; as regards the specific action of syphilis in producing scrofula I feel strongly disposed to contest it altogether. In 9,973 cases of all kinds of skin disease

¹ *Op. citat.*, p. 187.

in my department at St. John's Hospital I found 76 cases of scrofulo-derma. In almost every instance I inquired carefully into the antecedents of the parents, the fathers especially, without being able to find evidence of pre-existence of venereal disease in a single instance. One author has asserted that struma is always the offspring of syphilis. This means in plain English to brand thousands of fathers, men of irreproachable life, with a stigma of the worst kind; and as struma prevailed for ages in districts to which syphilis had never found access, I think we should commit no crime in assuming that the author of this statement, unsupported by any facts even in his own pages, was as far wrong as he could well be. The same doctrine was at one time upheld by some practitioners about lepra and with about the same amount of foundation. Indeed, by what process of reasoning those, who believe that syphilis only appeared towards the close of the fifteenth century, can regard it as a specific cause of lepra, a disease described hundreds of years before with sufficient accuracy to admit of its identification, and of struma, which certainly seems to have existed long previously, is an enigma, the solution of which I must leave to those more versed in deciphering such secrets than myself.

Whether such opinions as these really gain currency, whether they really exert any practical influence over the great business of medicine, that is to say the treatment of disease, or whether they are merely regarded in the same light as any peculiarity in the spelling or punctuation of the author might be, is a question about which works on medicine never enlighten us, seeing that, with one or two rare exceptions, no allusion is made to the opinions of any preceding author on the subject. A statement, calculated to affect the very basis of syphilitic pathology, linked with the innermost histologic evolution of this disease, is, without a breath of fact in its favour, received by successive generations of readers, each one of whom has been taught in his time that medicine is a science. This let-alone fashion of dealing with the question has, however, at any rate, one merit which cannot always be claimed for medicine. It is tolerant in the highest degree; each succeeding announcement of this kind is received with the same impartial silence, and an observer, accustomed to the strictness of exact

research, might here repose from the fatigue of examining facts and luxuriate in the contemplation of a branch of knowledge where matters are made so easy, that a theory is received without evidence and without opposition. Hensler contends that every man should be at full liberty to put forth his own hypothesis about the origin and nature of syphilis ; he speaks of it as a privilege which we have no more business to contest than we have to refuse him the right to call his house his own, or to sit under the shadow of his own fig-tree. We have gone beyond Hensler ; for we extend this latitude not only to theories about syphilis, but, as the reader has just seen, to those about the derivatives of this disease. But whether such tolerance has been for good or for evil, *whether in fact what is called science can afford to be tolerant*, is quite another question. For it is to be remembered that we are dealing here, not with popular errors such as the astronomer and chemist can afford to overlook, but with tenets calculated, if erroneous, to carry the seeds of decay into the innermost recesses of syphilitic pathology.

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Remarks
ON
HODGKIN'S DISEASE.*

BY

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ENLARGEMENT of visible lymphatic glands is a sign of disease quickly noticed by the sufferer and usually a cause of anxiety to him. In many cases it is also a cause of anxiety to the physician. It is true we are often able to reassure the patient—for instance, when it is recognized that the enlargement is due to septic infection within the lymphatic drainage system of the affected glands. Unless too far advanced, the cure of the original infective focus will cause the enlargement of the glands to disappear. In other cases we may be able to discover that the enlargement is due to a chronic infective process, such as tuberculosis, or to the occurrence of new growth, either primary or secondary in character. In such cases there is sufficient cause for alarm, but we have the advantage of understanding something of the nature of these enlargements and the satisfaction of knowing how much can be done and what should be avoided. But there are still other instances in which we are quite ignorant of the cause of the enlargement, but we do know the progressive nature of the disease. The most important example of this class is

* Sir James Galloway, whose death on October 18th has been so great a loss to clinical medicine, left behind him notes he had prepared for a post-graduate lecture he was to have delivered before the Fellowship of Medicine and Post-graduate Medical Association on October 11th. They have been put in order by Sir William Hale-White and are here published. It is probable that Sir James Galloway would have made the lecture longer had he survived.

Hodgkin's disease. A recent writer has stated that this disease "presents the most hopeless condition in the whole domain of medicine." It cannot be denied that there is much truth in such a statement; it may, we trust, prove to be too emphatic. In this lecture I propose to draw your attention to certain manifestations of this disease in the hope of stimulating interest and investigation.

It is clear that the serious nature of the malady and the mystery of its occurrence must have made an impression on the mind of Dr. Thomas Hodgkin when he wrote his paper on "Some morbid appearances of the absorbent glands and spleen"¹ in the year 1832. In the series of cases he describes he saw and noted the association of prolonged illness with enlargement of the lymphatic glands and the formation of new tissue resembling the lymphatic glands in the spleen. It cannot be said that his observations are presented in a very attractive form. Hodgkin apparently did not proceed with his investigation of the subject, and the record of his discovery appears to have been almost forgotten. Dr. Richard Bright² referred to Hodgkin's cases in his description of certain abdominal tumours, but with this exception Hodgkin's observation seems to have been lost sight of until Sir Samuel Wilks³ once more drew attention to the subject of "Enlargement of the lymphatic glands combined with a peculiar disease of the spleen" in 1856, and, recognizing the priority in discovery, spoke of "Hodgkin's disease." Wilks strangely had been in ignorance of Hodgkin's description during his earliest work on this subject, but at the end of his paper he writes: "It is only to be lamented that Dr. Hodgkin did not affix a distinct name to the disease, for by so doing I should not have experienced so long an ignorance—which I believe I share with many others—of a very remarkable class of cases, the recognition of which would have guided both myself and others to an explanation of some more recent cases coming under our notice." If Sir Samuel Wilks could now look back on the record of descriptions and investigations of this disease he would sympathize with the difficulty of affixing a "distinct name to the disease."

Since those days the disease has been carefully studied, using all the means at the disposal of modern investigation; its possible association with tuberculosis has been discussed—many have held that it is in reality an unusual manifestation of tuberculosis infection, an opinion probably now held by but few; the peculiar fever associated with the disease has been noted and looked upon as evidence that the disease is an unrecognized form of relapsing fever. Its relations to disorders of the blood, especially the varieties of leukaemia, have been investigated, and still we are unaware of the cause of the disease or its exact morbid relationships. The various synonyms which have been suggested are in some cases useful in laying stress on features of the malady; but it appears to me best still to adhere to the name of "Hodgkin's disease," especially as new "distinct names" do not take into account sufficiently the fact which becomes more evident—that we are dealing with a general specific disease associated with progressive infection of susceptible structures.

A very interesting chapter of medical bibliography could be written respecting Hodgkin's disease, especially if note were made of some of the distinguished writers.

The disease is so widely distributed, and its main features are now so well given in many textbooks, that it is unnecessary as well as inopportune for me to describe them in this place. I propose to illustrate points in its symptomatology, using for this purpose cases recently under my own observation. I think that the cases will support the conception of the malady which I have stated.

THE OCCURRENCE OF UNUSUAL PROTEIN IN THE URINE.

Apart from the complication produced by true nephritis, I believe that the occasional or temporary occurrence of albumin in the urine in small quantity is not uncommon in Hodgkin's disease, just as in many other chronic maladies, and is probably not of special significance; but the incident I am about to relate is in a different category. I have had recently under close observation, until the time of his death in Charing Cross Hospital, a case of the disease in a man. Early indications of the disease, such as enlargement of the cervical and inguinal glands, appear to have been noticed in the early part of 1919. He came under my close observation in 1920, and was then in fair health, although the nature of his malady was clear. In July, 1920, I examined his urine carefully; it had a specific gravity of 1.016, and contained no albumin and no sugar. I again saw him in the early part of 1921, when he told me that something unusual had happened; at night-time, he said, he was troubled by having to pass urine in large quantities, although during the day-time he was little disturbed in this way. On examining his urine on this occasion I found the specific gravity to be 1.014. On proceeding I saw the faintest possible cloud after acidulating the urine and heating. To my surprise this cloud disappeared on heating still further, and showed itself again on cooling. Treating the urine with picric acid saline I obtained a heavy flocculent precipitate estimated in an Esbach's tube to be about 14 parts per 1,000. It was now quite clear that the urine contained a protein of unusual character, and I suspected that it might be an example of "Bence-Jones albumin." Careful and repeated examinations showed that this peculiar protein was constantly present. It occurred in large quantities, varying from 1 to 2 up to 10 to 15 parts per 1,000 by Esbach's method. This peculiar protein remained present during the rest of his life: the symptom of polyuria gradually diminished, and in the later stages the amount of this protein present was considerably less than when first observed. Fortunately, when the patient had to be admitted to hospital I had the co-operation of my colleague Mr. Sydney W. Cole. The characters of the precipitate have been carefully investigated, both at Charing Cross Hospital and in the Biochemical Laboratory, Cambridge. I hope that Mr. Cole will publish shortly a full description of this unusual substance. In the

meantime, by way of identification, I can give the following description sent to me by Mr. Cole :

The protein is similar to the "Bence-Jones albumin" in that it is coagulated on heating, redissolves on further heating, and reappears on cooling. It differs in several respects :

<i>Bence-Jones Protein.</i>	<i>New Protein.</i>
Coagulates at temperatures under 55° C.	Under the most favourable conditions it does not coagulate under 75° C. Full coagulation at 79° to 82° C.
Coagulates at 55° C. in the presence of a minimum amount of salt and acid.	Only coagulates in the presence of a considerable amount of acids, and salts like sodium chloride or ammonium sulphate.
Difficult to coagulate.	Becomes quite insoluble if kept at 80° C. (in the presence of acid and salt) for some minutes.
Even when highly diluted exhibits a white ring of precipitate when poured upon strong HCl (Bradshaw's test).	Not precipitated by HCl under any conditions.

The new protein is best detected by noting the precipitate in the cold with sulpho-salicylic acid—the urine itself failing to give a heat coagulum under optimum conditions of reaction (that is, just acid to brom-cresol-purple). The new protein often separated in flocculi as the urine cooled.

Microscopic and other investigations of the urine were frequently carried out, and gave no evidence of true nephritis. The autopsy completely confirmed the diagnosis of the case. The kidneys were unusually affected; the capsules were greatly thickened, but stripped off easily. There were cysts in both kidneys; microscopical examination showed that the cortices of the kidneys contained small areas of the characteristic overgrowth of the disease.

THE FEVER OF HODGKIN'S DISEASE.

In all cases of this disease the body temperature is raised at some time. On looking over many temperature charts the occurrence of fever seems often to be irregular and comparatively mild in degree. But in a few cases the course of the temperature is extraordinarily regular, both in its exaggerations and in its relapses. In a characteristic case the chart is one of the most remarkable within medical knowledge.

The temperature of other cases, which appears at first sight to be irregular, on closer examination gives evidence of a definitely periodic course. The peculiar character of the temperature has attracted the attention of many observers; its occurrence was emphasized by Pel⁴ of Amsterdam, and by Ebstein⁵ of Göttingen, who described such cases as examples of an unusual type of relapsing fever.

I have recently had under my observation a patient whose temperature was a very perfect example of this relapsing type. I have a complete record of his body temperature during a period of five months preceding his death. After being nursed at home, and later in a private nursing home, he was admitted to Charing Cross Hospital under the care of

my colleague Mr. Peter Daniel. I had therefore frequent opportunities of seeing the patient during the last two months of his life.

On examination little could be made out in the way of physical signs. None of the easily palpable lymphatic glands were enlarged, although I suspected that the retroperitoneal lymph glands might be. These, however, were not easily palpable. After death the glands extending along the whole of the left side of the lumbar vertebrae formed a large mass, the glands in the groin were also enlarged, and there were white nodules in the spleen.

During the relapses of the fever he was deeply unconscious, with low delirium, and resembled a severe case of typhoid fever about the third or fourth week. When the temperature remitted he recovered consciousness to a very marked degree and improved greatly in general appearance. But even in the afebrile periods the patient was obviously seriously ill.

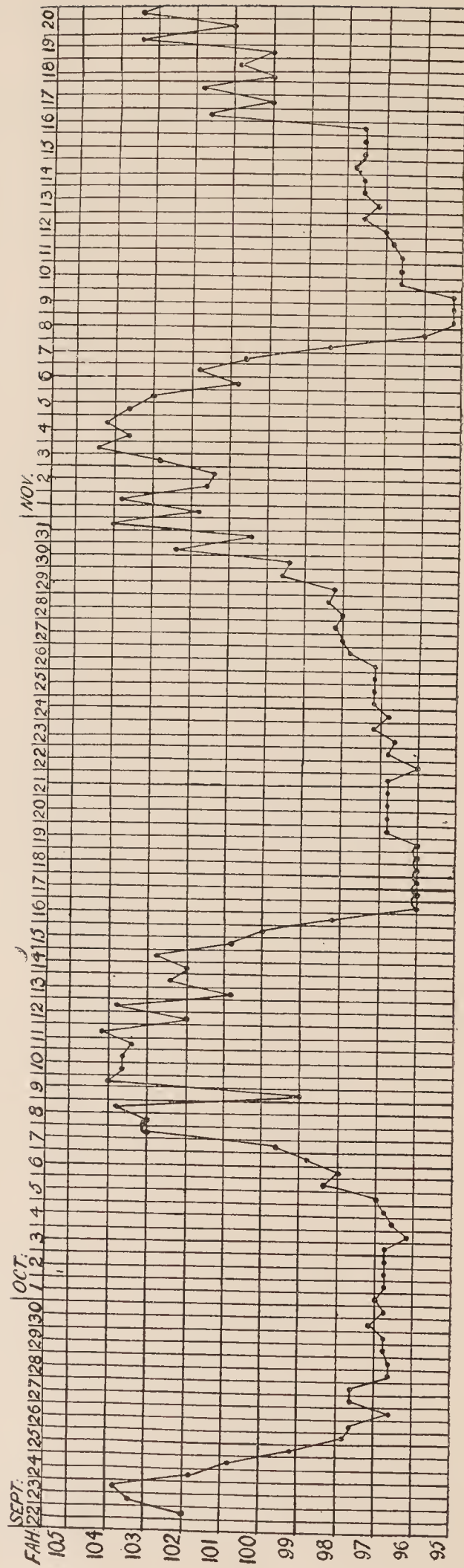
On studying the temperature chart we observe that the fever is markedly relapsing in type; there are febrile periods of from five to eleven days in duration, succeeded by periods of pyrexia usually longer than the periods without fever. The pyrexial period in what may be regarded as atypical outbursts lasts for about twelve days; the cycle of the disease, reckoning from the commencement of the afebrile period to the end of the period of pyrexia, appears to be on the average about twenty days.

The rise of the body temperature is rapid, reaching an elevation from subnormal or normal to 104° or 105° in the course of forty-eight to sixty hours. This rapid rise was associated with a certain amount of shivering and other evidences of a febrile attack, especially with rapidly increasing and severe toxaemia. During the period of raised temperature the patient was almost unconscious, as if severely poisoned; he could, however, be roused to answer simple questions, but with great difficulty.

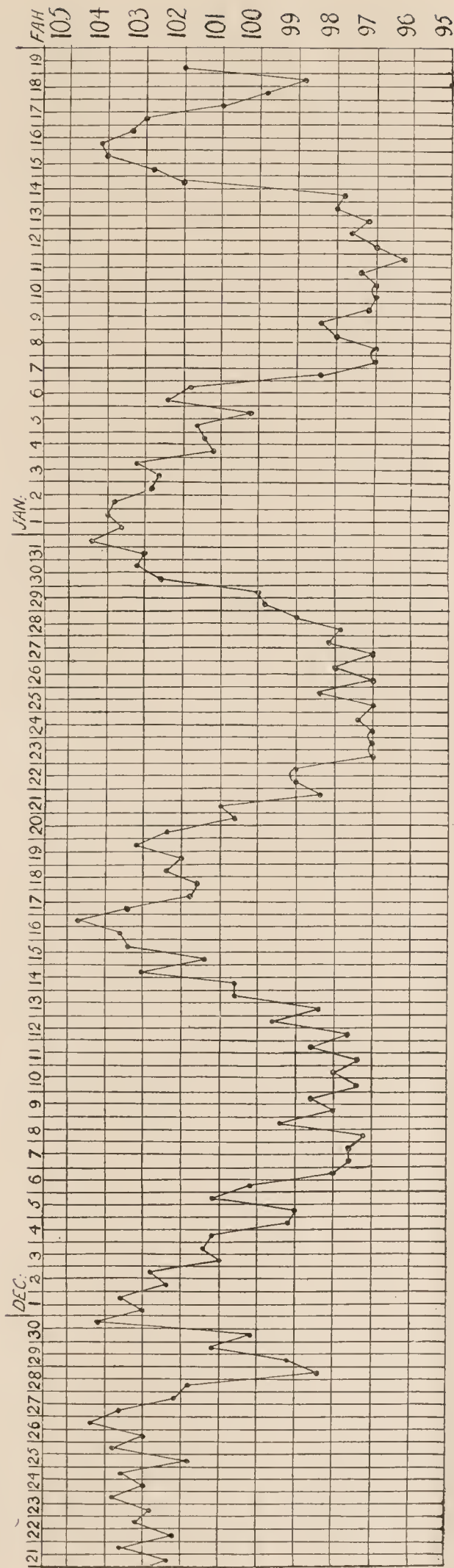
After the period of eleven or twelve days the fall of temperature took place, usually more rapidly than the rise. Thus a fall from 104° or 105° to subnormal ranges of 96° and 97° would occur within forty-eight hours. After four or five days of subnormal temperature the curve would again rise slowly towards the normal, then suddenly the upward curve would show itself, indicating the commencement of the period of relapse.

This temperature chart also suggests that two attacks of fever might succeed each other rapidly, so that there would be an unusually long pyrexial period of about twenty-one days, but during such a prolonged febrile attack definite remission of fever occurs, indicating the usual limit of a febrile period, succeeded immediately by a new access of fever. It was remarkable how rapidly the patient recovered consciousness when the temperature fell.

I have had this prolonged record of fever reduced from the four-hourly charts to a small scale of one-tenth of an inch to each observation (see Chart). As nearly as possible I have taken the temperature record of 8 o'clock in the morning and 8 o'clock in the evening as indicating the course of the fever.



TEMPERATURE CHART.



CONTINUATION OF TEMPERATURE CHART.

The reduction has brought about the appearance of exaggeration of the variations of temperature relatively to the time. Bearing this in mind the reduced chart demonstrates very well the peculiar relapsing nature and the severity of the fever. I owe to my friend Professor Arthur J. Hall⁶ of Sheffield the suggestion of arranging such prolonged temperature curves in the way described. Professor Hall showed me a chart arranged in this manner in a similar case of prolonged Hodgkin's fever.

On examining such a temperature curve we get a strong impression that the disease characterized by this fever is caused by a specific poisoning; also that this poisoning is of infective and in all probability of parasitic nature; and further, that the periodicity of the temperature depends on some such event as the life cycle of a parasite. It is a facile suggestion to make—that the infective agent may be a protozoon. Unfortunately, as yet we have no definite evidence that a parasite is the cause of Hodgkin's disease.

THE CUTANEOUS SIGNS OF HODGKIN'S DISEASE.

Probably in all infections certain tissues are more susceptible to the morbid influence than others. This is very clearly the case in Hodgkin's disease. The lymph glands and allied structures are peculiarly susceptible. It might at first be thought that as the skin does not contain obvious lymphoid structures, with the exception of the all-pervading lymphatic channels, it would not usually show signs of the malady, but the observation of patients teaches that the skin not unusually gives evidence of the disease. These manifestations fall into two categories. By far the most important is the appearance of small erythematous points or macules on the skin of the body and the extremities. Sometimes these become distinctly papular, raised a little above the surface; usually they disappear, leaving a faint staining of the skin. Very rarely these papular lesions become slightly vascular. In association with the enlargement of the lymphatic glands, the cutaneous condition closely resembles the severer forms of prurigo. As the result of these lesions, aggravated by friction and scratching, wide areas of the skin may become thickened and swollen, producing an exaggeration of the natural texture of the skin, emphasizing the folds and wrinkles, and giving an elephantoid appearance. This condition of the skin closely corresponds in appearance with what has been named lymphodermia. Coexisting with the above state the surface of the skin may be dry in some cases, desquamating in fine scales, and very often showing widespread pigmentation of a pale yellow-brown colour. But the most important of all the cutaneous symptoms is pruritus. I believe that the misery experienced by the itching in some cases of Hodgkin's disease is unexampled in other maladies. It seems to occur very commonly in mild degree, but in those cases in which the papular lesions develop the itching is intense and most exhausting. Nothing seems to be able to control the desire to scratch and rub the affected skin. The patients scratch open the papules, and only then will they admit that they have a certain amount of relief. Anyone who has to deal with

a severe case of Hodgkin's pruritus will not fail to remember the misery of the patient and the difficulties experienced in advising treatment.

The histological examination of the skin in such cases shows two sets of lesions. In the first there are the usual changes occurring in severe non-pyogenic dermatitis. Such changes might arise from the specific poison of the disease itself or as the result of absorption from the newly formed tissues of the disease. Secondly, in some cases the formation of new cells in the neighbourhood of the blood vessels and lymphatic channels seems to be of a special character and to resemble the characteristic overgrowth of the disease.

In addition to these generalized cutaneous conditions, which are not very uncommon, there occurs much more rarely the formation of small tumours and nodules in the skin. They are often numerous, small, up to the size of a broad bean, flattened rather than definitely raised from the surface, and pale pink or brownish in colour. The histological examination of these nodules shows that they are composed of the characteristic Hodgkin's "granuloma." These nodules may occur apart from the other indications which I have mentioned.

These cutaneous lesions are not only important in themselves, but they have a close resemblance to the skin manifestations of other diseases. The most striking of these is the distressing malady known as mycosis fungoides. Just as in Hodgkin's disease, mycosis fungoides seems to be caused by a general infection of the body. The cutaneous manifestations fall into two groups: a widespread inflammatory change in the skin, and the formation of tumours. The histology of the tumours closely resembles the histology of the granuloma in Hodgkin's disease, so much so that certain very competent observers have come to the conclusion that mycosis fungoides is but a special manifestation of Hodgkin's disease, affecting mainly the skin. It is very important as well as interesting to note that the cutaneous manifestations in the varieties of leukaemia also resemble in many of their features the conditions mentioned. There may occur a widespread pruriginous dermatitis on the one hand, and the formation of nodules composed of characteristic cellular elements on the other.

TREATMENT.

As we are yet ignorant of the cause of this disease, its treatment is unsatisfactory. There is still too much truth in the opinion that there is no recorded case of cure or recovery. There are, however, methods of alleviation.

Arsenic seems to be the only drug which has any beneficial influence. A considerable amount of evidence shows that the drug is of value, at any rate in certain cases. It has been used in various ways—by the mouth, subcutaneously, and, since the introduction of the salvarsan group of preparations, by intravenous injection. I have used the drug in a considerable series of cases, and have come to the conclusion that benefit is most conveniently obtained by its administration in the form of sodium cacodylate. The plan I usually follow is to prescribe the cacodylate in quarter-grain doses by the

mouth, repeated during the day till as much as a grain or a grain and a half is administered daily. The patients bear this preparation well, with little intestinal discomfort. It is usually an indication to stop administering the drug, temporarily, when the characteristic garlic-like odour can be appreciated on approaching the patient closely. As a result of the administration of the drug, I believe that I have seen the glands diminish in size and the patients become more comfortable. A point of some interest has to be borne in mind. It appears that in some of the cases who have developed the characteristic pruritus of the disease, arsenic, at any rate after a time, seems to increase the pruritus. On stopping the drug I have known the pruritus diminish in degree.

A second method of treatment of undoubted value in certain cases of the disease is the use of x rays. The most distressing and dangerous complications of the disease occur when the glands within the thorax are affected. They may increase in size, producing at first discomfort, then symptoms of serious dyspnoea with stridor, owing to the pressure of the tumours on the trachea, bronchi, and other structures. A good many cases are now on record showing that the careful use of x rays in such cases of mediastinal Hodgkin's disease has brought about a great improvement in the symptoms and much comfort to the patient. Not only so, but repeated x -ray examination of the chest in these cases shows very clearly that, at any rate, the shadow produced by the tumour is greatly shrunk, no doubt owing to diminution in size of the glandular masses.

I have under observation at the present time a lady who was brought to me with clear evidences of Hodgkin's disease. There were enlarged glands in the axilla and in other parts of the body, but the main seat of the disease seemed to be those in the right side in the mediastinum. The patient suffered from dyspnoea with severe exacerbations and much stridor, so that her life was in immediate danger. She was treated by my late colleague Dr. Ironside Bruce, more than two years ago, with very satisfactory results. The patient improved, the dyspnoea and other discomforts almost disappeared, and she was able again to undertake her duties in her house. More than a year after a recurrence of the symptoms took place and she was once more treated by Mr. Stanley Melville. The favourable results, with relief of the symptoms, once more occurred. The patient after two and a half years still remains free of her symptoms and is able to carry on her duties. (See Figs. 1 and 2.)

In this case an enlarged gland was removed from the right axilla and shows the characteristic histological structure of the disease. I am glad to be able to demonstrate the x -ray photographs in this case, before and after treatment, and I have also had the advantage of comparing with these the x -ray photographs in a very similar case with similar results treated by my colleague Dr. Russell Reynolds.

There is one observation I should like to make with respect to x -ray treatment of this disease—the lesson was strongly impressed upon me when treating some cases with the help

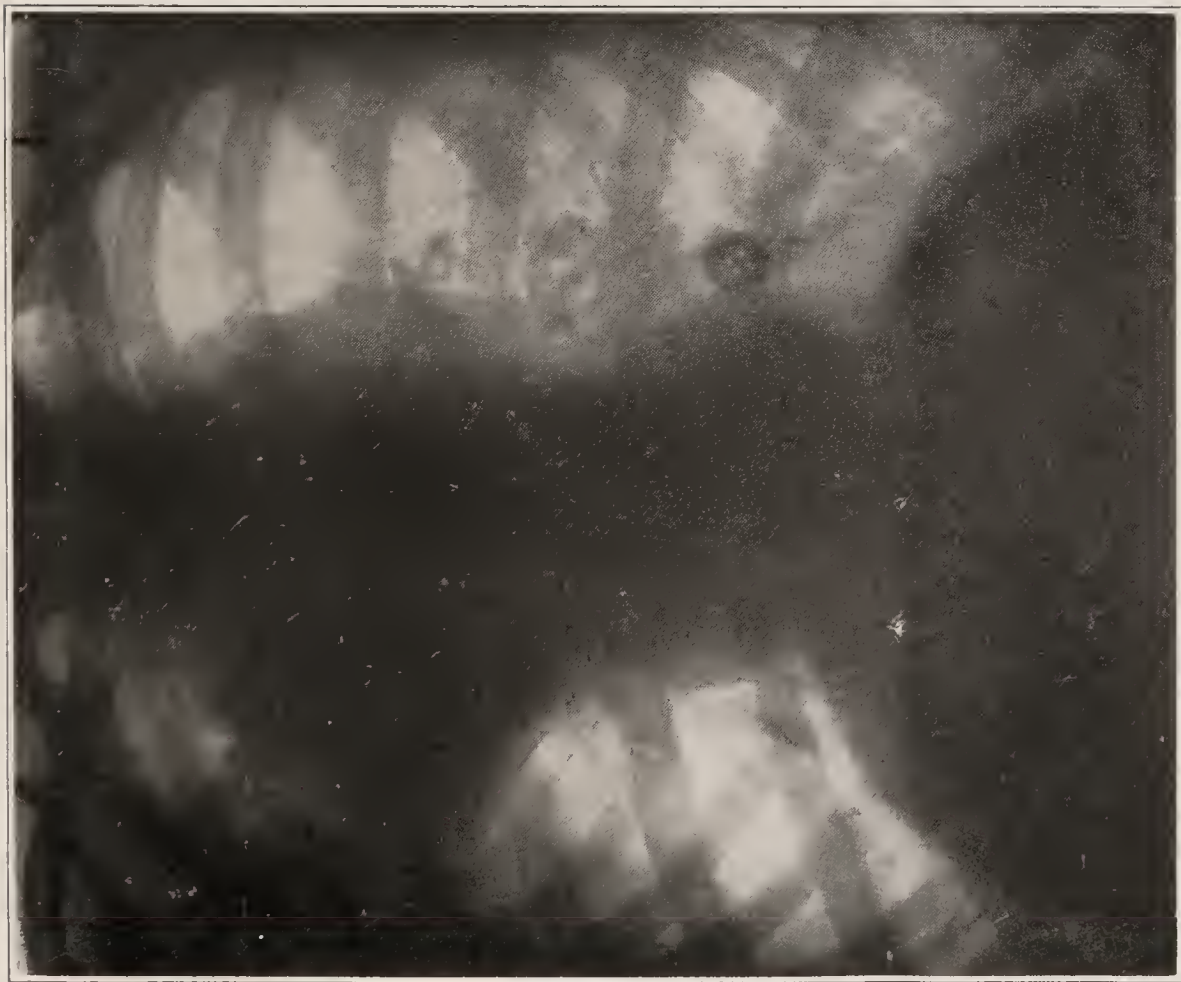


FIG. 1.—Photograph of chest on November 30th, 1921, showing the mass in the mediastinum before treatment by *x* rays.

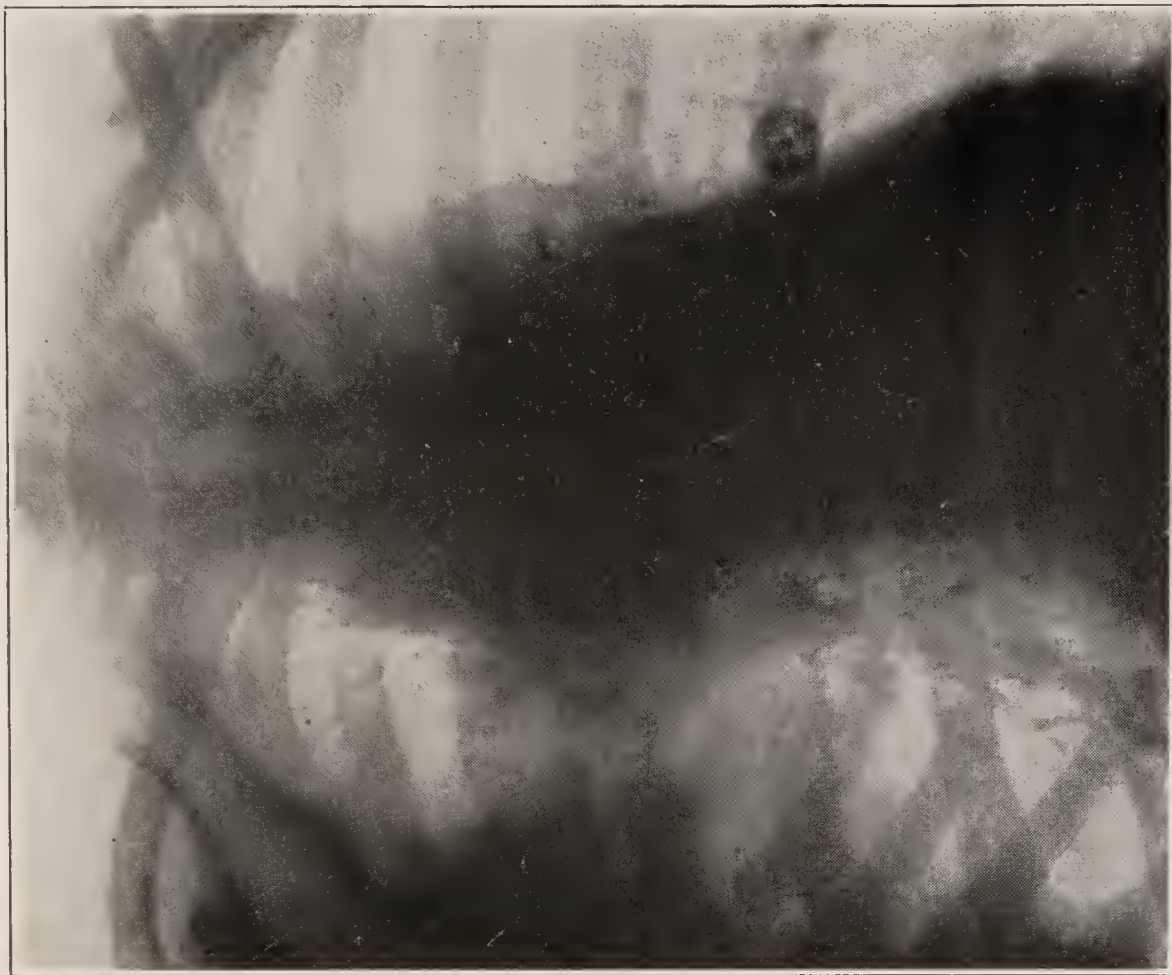


FIG. 2.—Photograph of chest, January 11th, 1922, showing diminution of the mass under *x*-ray treatment carried out since November 30th, 1921.

of my late colleague Dr. Ironside Bruce. It seems to be possible in certain cases, and apparently when too rapid resolution of the lymphoid masses takes place under x rays, that very severe reactions with serious rise of temperature and other untoward results may follow the x -ray exposures. This phenomenon suggests that alien protein poisoning occurs as the result of the breaking down and absorption of the newly formed cells. It must, however, be stated that the tumours of Hodgkin's disease do not always diminish under x rays. In some cases the application has little or no beneficial effect; it is possible that in these cases much fibrous change has taken place in the glands, so that little beneficial effect from x rays can be expected.

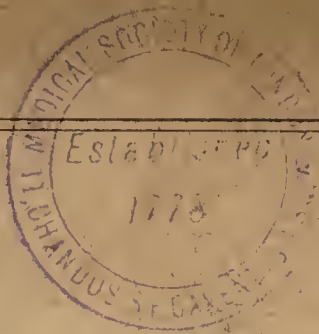
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[EDITORIAL COMMENT.]

A SAD interest attaches to the article on Hodgkin's disease published this week (p. 1201). Sir James Galloway was preparing a post-graduate lecture on the subject at the time when he was taken ill, and it was in his mind at the last. The notes he left have, as an act of piety to his friend, been put together by Sir William Hale-White, who has little doubt that the lamented author had intended perhaps to expand the lecture and almost certainly to complete it by some further discussion of the observations and theories he recorded. Even as it stands now it well illustrates Galloway's qualities as a physician and his attitude towards the more modern methods of studying disease. While he put first clinical observation, coupled in any particular case before him with a consideration of the individual patient, he always had in mind the institutes of medicine, and was ever ready to study the suggestions continually flowing from physiology and pathology into the broad stream of medicine. The lecture affords more than one illustration of this. The discovery that the urine in the first case he relates contained a peculiar protein at once aroused his attention. He recognized that it resembled Bence-Jones's albumin, and sought the expert help of Mr. Sydney W. Cole, who established the fact that the substance was a new protein, differing from Bence-Jones's albumin in the respects briefly stated in the lecture. We may express the hope that Mr. Cole may shortly publish a fuller account of it, and that other physicians will look out for it in similar cases. Early in his career Galloway had given special attention to diseases of the skin, and the descriptions in this lecture of the cutaneous manifestations to be observed in Hodgkin's disease are of great value; commonly these manifestations are mentioned only briefly in text books, and yet they may be of very considerable importance. That the pruritus occasionally observed may be so intense as gravely to

affect the patient's general condition is a valuable observation. The further statement that arsenic, a favourite remedy in the disease, may increase the pruritus, is a useful clinical point. The etiology of Hodgkin's disease is still obscure; Galloway, in pointing out that it is a general specific disease associated with progressive infection of susceptible structures, ranged himself with those who hold that the poisoning is of an infective and, in all probability, a parasitic nature. This view is supported by the occurrence of pyrexia, of which a very good example is given, the temperature being plotted in the way recently suggested by Dr. Arthur Hall in the *Quarterly Journal of Medicine*. The periodicity of the temperature may depend on the life cycle of the hypothetical parasite. Finally the observations on the value of *x*-ray treatment are worthy of note: others also have seen the treatment do great good, and it seems clear that its application to the glands of the chest may relieve urgent symptoms.



The Natural History and Epidemiology of Cholera:

Being the Annual Oration of the Medical Society
of London, May 7th, 1888.

By SIR J. FAYRER,

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1888.

THE NATURAL HISTORY AND EPIDEMIOLOGY OF CHOLERA.*

MR. PRESIDENT AND GENTLEMEN,—It is in obedience, sir, to the behest of your predecessor that I occupy this evening the post assigned to him who, in compliance with ancient usage, is entrusted with the duty of commemorating the inauguration of the Medical Society of London by delivering an annual oration which shall deal with matter germane to the purposes for which the Society was founded.

I am as deeply impressed with a sense of the honour conferred on me in being selected for this duty, as I am with that of my inability to do it the justice it merits. I can only, at once, offer my grateful acknowledgments, and crave indulgence for the shortcomings from which I cannot hope to escape.

The subject I have selected to bring before you is "The Natural History and Epidemiology of Cholera." It appeared to me, after revolving in my mind other possible subjects for this address, that I could hardly select one of greater interest at any time, but especially now, that the dark shadow of this mysterious pestilence which has so recently loomed over Europe, has passed away, after threatening, though not invading, our own islands—thanks, no doubt in a great measure, to the protection afforded by a system of sanitary administration which, whilst preserving the general health of the people, has rendered them less susceptible to disease, the local causes of which it has contributed to diminish, if not to destroy.

I do not propose to dwell on the pathology or thera-

* Being the Annual oration of the Medical Society of London, May 7th, 1888.

peutics of cholera, but to submit to you the views which, to me at least, seem most in accordance with other facts concerning it that have been ascertained. I purpose, in short, to give a brief review of its history, habits, method of diffusion, geographical distribution, relation to climate, season, meteorological conditions and locality, its etiology, its effects on the human race, and, finally, the methods which experience has taught us are most efficient in mitigating or preventing it. This involves so much that I cannot hope to do more than indicate the most prominent points of each of these subjects; still I trust I may be able to interest the Fellows of the Society and the visitors who honour us with their presence this evening.

The subject has for many years interested me and occupied my attention; whether in the West Indies; the epidemic of 1849 in England; in India and Burmah during a varied experience of nearly a quarter of a century, or as a member of the Army Sanitary Committee for the past sixteen years, when the effects of cholera on the army as well as on the vast civil population of India have been constantly before me in the exhaustive reports which are regularly published by the Governments of India. On the ground, therefore, of personal experience, I venture to think I have some claim to make cholera the subject of this address; and, as regards fitness in respect of time and place—bearing in mind the ever-increasing tendency of cholera to enlarge its range of geographical distribution; the fact that it has so recently been present in Europe, threatening, though happily not actually invading, our own islands; that it has been the subject of international conferences, which have resulted in little else than to leave England hopelessly at issue with other powers in respect of the methods of prevention or protection; and, further, considering that as in England and India the measures adopted are totally different to those of other nations, but, as we are confident, productive of the best results—I think that no more fitting occasion for a review of the whole question could be found than the annual oration of the Medical Society of London.

My subject, therefore, is the natural history of a pestilence which exhibits many characters in common with the plagues of the middle ages; like them traversing the earth in zones, spreading in tropical, temperate and polar regions, attacking all sorts and conditions of men, uncertain and

often apparently capricious in its incidence, terrible from the rapidity and intensity with which it strikes, and from the obstinacy with which it resists all therapeutic measures, yet at the same time obedient to certain laws which regulate its incidence, diffusion and decline. Of its cause, if indeed we may assign it to any single cause, we are still ignorant, but experience and observation have made us so far familiar with its habits and the manner of its propagation and diffusion, that we are able to say how its incidence may be evaded, its course stayed, its rigour mitigated, and how it may be disarmed of much of its terror; nor are we without hope that in time to come it may, like the black death, the sweating sickness and other pests, give way before the application of the laws of hygiene, and take its place among the records of the past.

Having much in common with other epidemics, cholera possesses well-marked features of its own, but there is some reason for believing that it may have close etiological affinities with other diseases which in many respects differ from it widely in their characters.

No disease better illustrates the peculiarities of an epidemic; diffused far and wide over extensive countries, often leaping from one to the other, as it were, by bounds, or spreading rapidly among more limited communities, following a definite track, modified by climate or geographical position, dying out gradually or rapidly, to become extinct for a time, or to remain in abeyance till revived into activity by fresh influences. On the other hand it may occur in the sporadic form, or prevail as an endemic in certain regions from which it is never absent (such as in what is called the endemic area of Bengal), whence it may spread epidemically to regions beyond.

History of Cholera.—Cholera is an ancient disease; as far back as the records of medicine extend, descriptions of it are to be found. It has been said that it first appeared as an epidemic at Jessore in Bengal in 1817, but, as I need hardly say, this is not the case. It is described by Hindoos, Chinese, Greeks and other ancient writers of the pre-Christian era; by Romans, Greeks, Arabs, and a long succession of other authors up to the present day. The Ayurveda of Suscruta describes it as Visuchika;* Chinese writers, contemporaneous with Hippocrates (5th century before Christ),

* Macpherson. "Annals of Cholera,"

mention it under the name of Ho-louan; Hippocrates speaks of it as *χολερη*, Ionic form of *χολερα*, from *χολη*, bile, and *ρδία*, flux, or perhaps *χολερα*, the gutter of a house.* He gives descriptions of certain cases and alludes to seasonal prevalence, but does not refer to it as an epidemic. Like the Chinese he speaks of two forms, the wet and the dry. The Arabic names, "Wubba" and "Taoun," though applied to cholera, also mean pestilence, whilst "Haiza," the term used by Rhazes, Avicenna and Averrhoës,* is that in common use in India at the present day. There are various names for cholera in the East, most of them, significant of the characteristic symptoms.

Cholera is mentioned by Celsus, Aurelianus, Aretacus of Cappadocia, Paulus Aegineta, Alexander of Tralles; by Arab writers, Rhazes, Avicenna, Averrhoes, by Ali Ben Hossein of Bokhara (1364), and Mahmud Ishah.* Bernard Gorden, John of Gaddesden, Raphael of Volterra and others mention cholera as a well-known disease in Europe, but the 13th, 14th and 15th centuries furnish little information on the subject.*

In Elliott's "History of India," a disease which may have been cholera is mentioned as occurring in 1325, but there is no other notice of it in India by Europeans before 1503.* In Europe, from early in the sixteenth century there are notices of epidemics of bowel affections and of what is called "trousse-galant," which appeared in England and France in 1545. In 1564 an epidemic of cholera occurred at Nismes; in 1643 and 1665 in Ghent, as described by Van der Heyden.* Piso says cholera was severe in Brazil in 1658,* Sydenham writes of an epidemic of cholera in London in 1669-82.† Dr. Macpherson, the learned historian of cholera, says it was present in various parts of Europe in a mild epidemic form during the eighteenth century, dying away towards the end and remaining quiet during the first years of the present century.* Outbreaks seem then to have been less severe, but the records of disease were very imperfect in those days. Sir J. Pringle describes it as prevailing in the Low Countries, about Ghent, towards the end of the eighteenth century. Dr. Short speaks of an epidemic in England in 1726. In 1722-23-24 it was in North Germany; in 1736 at Nimeguen;

* Macpherson. "Annals of Cholera."

† Sydenham's Works, translated by Swan, page 133.

in 1742-50 in Minorca (Dr. Cleghorn); in 1751 Malouin describes an epidemic in Paris; in 1767 Dr. Short mentions cholera making havoc among men. Dr. Holmes, President of this Society, in 1777, in an address to the Society, said that it came round every year as regularly as autumn, and I might give many other references about this period and later, bringing up a continuous history of cholera in Europe to the present time.

In Asia in the sixteenth century cholera was described by the Portuguese; it ravaged the troops of the Zamorin; and an epidemic which occurred in Goa in 1543 was described by Gaspar Correa,* who says the name given by the Portuguese was Mordeshee, which continued to be used under the forms Mordshi, Morshi, Mordeshin, Mort de Chien. Garcia d'Orta and Bontius give a full account of the disease in 1629 in Goa and Java; Linschott and others also mention it. Zacutus Lusitanus speaks of its prevalence in Arabia; Baldaeus, a Dutch clergyman, refers to fatal cramps in his account of the coasts of India in 1641; Cleyer noticed cholera in China in 1669; Thevenot in Surat in 1666. Then-Rhyne refers to a remedy for it in Japan. In India it was epidemic in Mewar in 1661, in Marwar in 1681-82, in Goa in 1683-84.* In 1757 cholera occurred at Tinnevely; in 1768-9 there was an epidemic in Pondicherry and on the coast, and in Ganjam and Calcutta in 1781. It appeared also in Java, China and the Mauritius, on the Malabar coast in 1782, as far south as Trincomalee; in Hurdwar and Central India in 1783, at Travancore in 1792, in Mewar and the Mahratta country in 1794.* After the last mentioned epidemics, notices of the disease become rarer until the great epidemic of 1817. There is abundant evidence to show that the disease has been well known and described since the very earliest periods of history, nor is there anything in this record to prove that its origin is to be traced to India alone.

The history of the distribution subsequent to this may be summarised according to Hirsch in a series called by him *Pandemics*.†

The first pandemic (1817-23), notably intense about Jessore in Bengal, extended over the whole of India.

* Macpherson. "Annals of Cholera."

† Hirsch. "Handbook of Geographical and Historical Pathology."

Taking a southerly direction it appeared in Ceylon (1819), and Bourbon and the east coast of Africa (1820). Its progress in an easterly direction began with Nepaul, thence to Arracan, Burmah, Siam, the peninsula of Malacca and the East Indies (all in 1819), the Moluccas, Philippines and Chinese Empire (1820) and Japan (1822). The first place to the west of India in which cholera appeared was the east coast of Arabia (1821), then Mesopotamia, Persia, along the coast to the Euphrates and Tigris, and thence to Bagdad and Syria (all in 1821). In 1822 it extended along the Tigris to Kurdistan, thence to Syria, Palestine and Damascus (1823), and from Persia to Russia in 1822.

In the second pandemic (1826-37) cholera advanced from Bengal, along the Ganges, through the North-West Provinces, and westwards in two directions; through Cabul, Balkh and Bokhara to Orenberg, where it died out the next year; through Mesopotamia, Arabia, Syria, Palestine and by Suez to Egypt, the north coast of Africa, the east coast, Abyssinia, and some of the Soudan countries; and, in the other direction through Persia and Transcaucasia to Astrakhan, and thence over Russia. It reached Germany through Poland and Danzig; Austria, through Galicia, Turkey and Asia Minor. It appeared in Great Britain, France and the Netherlands almost at the same time (1832); the next year it was in Spain and Portugal; in 1835 it attacked the south of France and Italy, and in 1837 was in Switzerland, Austria and Germany, attacking districts in the two latter countries which had escaped before. Norway and Sweden were attacked in 1834.

Cholera appeared in Canada in 1832, extended up the St. Lawrence river and through Detroit to the United States, along the east coast and down the Ohio. From New Orleans it extended through the southern, central and western States to the shores of the Pacific (1833), Mexico and the West Indies (1833); appeared in South America (Guiana) in 1835 and Central America in 1837. Manifesting itself in an easterly direction it appeared in China and Japan.

During the third pandemic (1846-63) cholera was widely prevalent in India and had appeared in Further India, the Philippines, China and Persia before the date mentioned. From Persia it extended by its former route to Orenberg, through Siberia to the shores of the Black Sea and Constantinople; it spread over a great part of Turkey, the

Danubian Principalities, Hungary, Asia Minor, Syria and Egypt, and reached ultimately the north coast of Africa (1848-49-50). At the same time it re-appeared in India, Further India and the Malay Archipelago, and attacked Greece and Malta. Meanwhile cholera had reached European Russia through Astrakhan, extending up the Don, thence over the whole country on to Germany (1848). It was in England, the Netherlands and Belgium in 1848-49, Sweden, Austria, France and Italy in 1849 and 50. It appeared in North America in 1848, breaking out in New York and New Orleans simultaneously, extending over all the States east of the Rocky Mountains and reaching Canada in 1849 and the west coast in 1850. Panama and Mexico were attacked in 1849, then South America (New Granada), and finally the West Indies (1850-54).

There was a remission from 1850-52, and after that date, in Europe, cholera appeared in all the countries it had visited before, with the addition of Spain and Denmark. In Asia it extended over the East Indies, China and Japan, Persia, and thence to Syria. In 1853 it appeared in Algiers, in 1855 in Egypt, and subsequently Nubia and the northern coast of Africa, Somali Land, Madagascar, Mauritius (for the first time) and the Comoro Islands.

In North America it was not severe, but in Central America its area was widely extended, and in South America, Granada, Guiana, Venezuela and Brazil were invaded.

The fourth pandemic (1865-75), unlike the others, took a westward course through Arabia and Suez; Malta, the South of France, Spain and Italy being the first places attacked. From Turkey, attacked in 1865, cholera invaded the countries of the north and east of Europe, attacking them almost simultaneously, appearing in England and Belgium in 1865, and subsequently breaking out in Switzerland, the Netherlands, Norway and Sweden.

In the western hemisphere it appeared first in the West Indies (1865), and in North America in 1866. In a northerly direction it extended over the United States from New Orleans to Nova Scotia, and in a southerly direction to Central America. In South America it first appeared in Paraguay, extended down the Uruguay to Buenos Ayres and in a northerly direction to Brazil; the Argentine Republic, Bolivia and Peru were invaded.

Meanwhile cholera had reached Persia, Mesopotamia and

Syria, and in the other direction Egypt, the northern coast of Africa and Senegambia. After ravaging Somali Land, it appeared in the interior, and later on the Mozambique coast and Mauritius. In an easterly direction it attacked the East Indies, China and Japan at an early period of this pandemic.

There was a remission during the years 1869 and '70, except in Russia and Persia; the Danubian Principalities, Austria, Turkey and Prussia were again attacked, but the south and west and the Scandinavian kingdoms suffered little during the second period. Cholera appeared in New Orleans in 1873 and extended over the central plain of North America; South America was entirely free.

In Asia cholera attacked Arabia and extended to Nubia, and, following the course of the Tigris and Euphrates appeared in Mesopotamia. It broke out in Turkestan and Bokhara in 1872, and in Syria in 1875.

The pandemic which began in 1883 is so recent that I give its history in fuller detail. During 1883 cholera was restricted to Egypt. The entire mortality is not given, but up to the end of July the deaths notified to Sir G. Hunter were 12,600—the real number being probably about twice that amount. The condition of the country is described as extremely insanitary.

In 1884 cholera appeared at Toulon on June 18th, and a week afterwards at Marseilles, subsequently attacking many towns—Arles, Aix, Perpignan, &c.—in the south-east of France, where it continued till the middle or end of September.

During July it was gradually increasing in France, and appeared in Russia in a mild form at St. Petersburg and Charkoff.*

In the beginning of August cholera was in Lombardy, and by the end of the month was diffused over great part of northern Italy, raging most severely in Spezzia.

In September it appeared in Naples, and was present there in a virulent form throughout the month. In Italy during the year there were 27,030 cases and 14,299 deaths.

In October cholera was dying out in all the districts attacked, but at the beginning of the month it broke out at Yport in Normandy, was reported in other parts of

* Cuninghams. "Cholera—What can the State do to prevent it?"

northern France, including Nantes, and finally appeared in Paris on November 5th, where it was active till the end of the month, there being during that time in the city 971 cases and 866 deaths.

During 1884 cholera appeared in two English ports, Cardiff being one, but failed to spread.

In 1885 cholera was prevalent in Spain from June to November, and extended over nearly the whole country. It was first reported in the provinces of Valencia and Castellon during the last week of March; by the end of May it began to diffuse, attacking Madrid in June, and spreading to the provinces of Saragossa, Toledo and Alicante. By the end of the month the mortality had reached 5,700.

During July many more provinces were involved, and the disease became much more serious in districts already attacked. The mortality for the month was not far short of 24,000.

At the beginning of August the epidemic was still increasing, but by the 7th it had reached its height, and declined steadily during September. The mortality for August was 45,000 at least; for September, rather more than 13,000. Twenty-four deaths took place within the British lines at Gibraltar.

The recorded deaths from cholera in Spain were 79,490, but 100,000 is nearer the real number. Valencia (13,400) and Saragossa (10,954) registered the greatest number of deaths.

Cholera appeared in August at Marseilles and Toulon; in November in Brittany—Brest and the immediate neighbourhood being affected.

Meanwhile, in September it had appeared in Parma, where there were 313 cases and 202 deaths; in Ferrara, Reggio, Massa, Rovigo, Genoa, Modena and Venice; during this year, however, in Italy, the disease scarcely reached the height of an epidemic. In Sicily cholera was prevalent during September and October; in the whole island there were 6,397 cases and 3,409 deaths, of which 5,535 cases and 2,959 deaths took place in the town and province of Palermo.

In Europe during 1886 cholera was prevalent in Italy, in the Austro-Hungarian Empire, and to a slight extent in Spain and France.

At the beginning of the year there were a few deaths

reported from Venice and the south of Spain, but cholera as an epidemic did not take any hold on Europe till the middle of April, when it first appeared at Brindisi, and almost at the same time in the Venetian province and to a slight extent in Brittany, while there was a recrudescence in Bilbao.

In Italy the province of Naples was slightly affected, but the greatest severity was felt in the north-east, in the provinces round Venice, as Ferrara, Padua, Bologna, Vicenza and Ravenna, and in the neighbourhood of Brindisi—Bari, Barletta, San Marco, Acquaviva suffering severely. The epidemic gradually increased in severity and in the range attacked till about the middle of August, the deaths up to that time being about 5,465. It then began to diminish in severity, but in September there were a few cases in Rome, and Sardinia was invaded. The epidemic may be said to have ceased in Italy by the middle of October, 21,000 cases and 8,650 deaths having been recorded.

The most striking feature of this epidemic was a severe outbreak at Francavilla Fontana in July, which suffered more severely than almost any other town in Italy since 1884.

In the Austro-Hungarian Empire cholera appeared at Trieste in June, and afterwards attacked places in Istria, Carniola, Dalmatia, Croatia, Bosnia and Servia. It appeared at Raab in September, and shortly afterwards at Budapesth, where it had caused nearly 500 deaths by the end of November. It then began to abate, but cases were still heard of up to the very end of the year, and there was a slight outbreak in Bulgaria (Tirnova) in December.

In Spain cholera was comparatively inactive. During the first three months of the year there were cases at Tarifa, near Gibraltar, and then cholera died down again till October, when there was a recrudescence in Malaga.

In France cholera was limited to the province of Finisterre, where several deaths occurred during the first four months of the year.

In Japan it is reported that there were 50,000 cases and 35,000 deaths; the Corea is said to have been decimated.

In South America cholera appeared in Buenos Ayres in November.

In India there was no exceptional prevalence.

At the beginning of 1887 Europe was free from cholera;

in March it was reported in Sicily, but did not acquire serious proportions till July. In that month it attacked Rocella on the Calabrian coast, and increased in Sicily and on the mainland.

In Sicily its range was limited to the provinces of Palermo, Messina, Caltanissetta and Catania; its severity fluctuated slightly, but there was no distinct abatement in the island generally till the beginning of October.

In August there were cases at Malta; cholera began to increase in range in the south of Italy, and attacked Naples, Resina and other places along the Bay of Naples. It increased in range along the Bay and in the extreme south till about the middle of September, and then Naples, Reggio and their surroundings became its chief seat. From that time it ceased to increase, and by the middle of October there were no further returns from Italy.

The greatest severity of the epidemic was at Malta; it was there steadily maintained during September and October, but then the decline began, and after the middle of November there were no further returns.

The total number of deaths in Italy (including Sicily) was 2,200, in Malta 429.

In South America, the epidemic, which began in November 1886 at Buenos Ayres and Monte Video, attacked the provinces of Uruguay, the Argentine Republic and Paraguay, and in the west Chili (for the first time), where it was limited to Santiago. By the middle of May it had ceased as an epidemic, but Santiago was again attacked in the middle of November, and Valparaiso became infected.

In India there was a severe epidemic in the north-west, and 70,000 deaths are reported in the North-West Provinces during June and July=1 per cent. In Peshawar city there were 280 deaths during the month ending in the middle of August.

From this period cholera seems to have been dormant in Europe. Whether there may be a recrudescence in the spring, time will shew.

Cholera has visited our islands several times as an epidemic, with the following results:—

Date.					Deaths.
1831-32	52,547
1848-49	53,293
1853-54	20,057

Date.	Deaths.
1859 (In an epidemic form cholera was limited to Wick in Caithness, Glass Houghton, near Pontefract, and Netley)	
1866	14,378*

Geographical Distribution, Habits, Conditions, and Epidemic Movement.—The foregoing account of its movements shows how widely cholera has extended over the earth's surface, but there are geographical areas which have not yet felt its malign influences. In some it has never appeared; in others, its incidence has been so slight as to amount practically to exemption. These regions (according to Hirsch and Cuninghame) are—

The whole of Oceania, except perhaps the north-eastern part of Australia, Tasmania, New Zealand, Fiji and the Malay Archipelago.

In Africa, the east coast south of Delagoa Bay; southern and central divisions of the interior up to the Soudan; the west coast up to the Rio Grande; Ascension.

In North America, all the country north of the fiftieth parallel; in the West Indies, Martinique.

In South America, the South Polar Lands, the Falkland Islands, Terra del Fuego, Patagonia.

In Europe, Iceland, the Faroë Islands, the Hebrides, the Shetland and Orkney Islands, Lapland, Russia north of the 64th parallel.

In Asia, the northern governments of Siberia and Kamtschatka; it is uncertain about Mongolia and Manchouria.†

In India cholera has either not visited, or but very slightly;—

The Andaman Islands, Mussoorie, Montgomery, Mooltan, Muzzafargurh, Dera Ghazi Khan, Sialkot and Nowshera‡

European towns that have hitherto practically escaped are:—

Wurtzburg, Frankfort-on-Main, Olmutz, Falun, Rouen, Versailles, Lyons, Sedan, Cheltenham.

On the other hand there are places from which cholera is never absent, and these endemic areas (according to

* *Pall Mall Gazette*, extra, August 8th, 1883; Macnamara, "History of Asiatic Cholera"; Lawson, "Lecture on Cholera."

† Hirsch. "Handbook of Geographical and Historical Pathology."

‡ Cuninghame. "Cholera—What can the State do to prevent it?"

Corre*) comprise India, Indo-China and perhaps a part of the islands of the Malay Archipelago, a vast assemblage of countries to which one might give the name of Indo-Malayan, and which corresponds to one of the hottest and dampest zones of the world; its limits would be on one hand the tropic of cancer and the 10th degree of south latitude; on the other hand the 65th and 125th degrees of east longitude.

In India itself, which is regarded by many as the sole birth-place and home of cholera, it is not by any means confined to Lower Bengal and the Delta of the Ganges as is sometimes stated, but is endemic in several other and widely-distributed areas; the deltas of the Brahmaputra and Mahanuddy; the interfluvial tracts of Behar; the deltas of the Irawaddy, Salwin, Godavery, Kistna and Cavery; the Konkan and Malabar coasts; the southern half of the North West Provinces and Oudh; the Gurgaon, Delhi and Karnal districts between the Jumna and Sutlej; the Kangra, Gurdaspur and Amritsar districts between the Beas and Ravi; the Hoshiapur and Jullundur districts, between the Beas and the Sutlej;† the cities of Madras and Bombay,‡ the valley of the Nerbudda and Tapti rivers.§

Hunter's investigations show that cholera is endemic in Egypt;¶ in parts of Russia there can be little doubt that it is so. It is continually present in England, as seen by the Registrar-General's returns, and probably in many other countries, though the mortality is seldom so high as to attract notice, except when localizing causes and epidemic influence co-operate to develop an epidemic.

Cholera then occurs in the sporadic, endemic or epidemic form; in the former it may appear anywhere; it is endemic in Bengal and other localities, whilst under certain conditions it rages from time to time over various parts of the world, like fever, plague, dengue, small-pox and others, including the great epidemics of the middle ages, some of which are now all but, if not quite, extinct.

The term epidemic influence is, I fear, but an expression of ignorance; we understand it to mean those causes

* Corre. "Traité Clinique des Maladies des Pays Chauds."

† Bellew. "The History of Cholera in Egypt."

‡ Aitkin. "The Science and Practice of Medicine."

§ Macnamara. "A History of Asiatic Cholera."

¶ Hunter. "Report on Cholera in Egypt, 1883."

external to the individual or the locality, whether of atmospheric or telluric origin, by which disease is diffused generally. Dunglison called it the epidemic constitution; Chevers says he believes "that the essentials to its occurrence (*i.e.*, epidemic cholera) are an atmospheric or telluric condition due, it may be to some undetected abnormality in the air or in the earth—electric, volcanic, or other—or to the presence of some undetected microzyme or germ which predisposes those who are within the area of its occupation to cholera.*

Leon Colin describes it as "a something isolated, impersonal, detached from the disease itself, the epidemic genius (constitution, influence), a certain creative force of the different epidemics, compelling, directing, extinguishing them."

Dr. Lawson speaks of pandemic waves in relation to the operation of this influence over the earth's surface in certain directions of a definite character, these directions being northwards. From further observation he concludes that the character of the waves is telluric (magnetic) rather than aerial; that they correspond for fever and cholera with this difference, that the minimum curve of one coincides with the maximum of the other.† He further remarks, "It may be said we do not know the intricate nature of gravitation, but we recognise its existence, and have become acquainted with the conditions under which it operates."‡ So it is with regard to epidemic influence.

Dr. Austin Flint says, "the morbid agents must be transported through the atmosphere or brought in some way from situations more or less distant. The causes of epidemic disease are migratory. In some instances they traverse almost every portion of the habitable globe. This is true of epidemic bronchitis or influenza, as of epidemic cholera. It is altogether improbable that the special causes in these and other epidemics originate in the different sections of country over which their prevalence extends."¶ Dr. Flint clearly thinks that the epidemic influence is an entity of some minutely particulate form, though he does not say what.

* Chevers. "Cholera Asiatica Maligna."

† Lawson. "Pandemic Waves."

‡ Lawson. "The Sanitary Lessons of Indian Epidemics."—*Medical Times and Gazette*, August 4th, 1883.

¶ Flint's "Practice of Medicine."

Aitkin says "there must be some distempered condition of the circumstances around us—some secret power that is operating injuriously upon our system—and to this we give the name of *epidemic influence or constitution* which is believed to predispose towards the receptivity of specific disease poisons."*

These definitions, however, help us but little. The fact is we do not know the real nature of epidemic influence; we only know that there is a potent factor in the diffusion of disease, whether it be a dynamic agency, an altered constitution of the atmosphere, or a widely diffused miasm of particulate form spreading far and wide over the earth's surface, as did the volcanic dust from Kratakoa, which but recently girdled the earth. It may depend on certain states of the atmosphere, deficiency or excess of electrical or magnetic tension, different degrees of moisture, of ozone, or other modification of its physical properties; something propagated in aerial or telluric currents, recurring at intervals, co-operating with local and personal causes, and conferring on the disease its quality of epidemicity. In some cases, perhaps, not only acting as the propagating agent, but as the cause itself.

Mr. Glaisher took the first steps in bringing to notice meteorology in its relation to epidemics, by his observations during three cholera epidemics in this country. This department of science is progressing, and data are likely to be furnished by well-organized meteorological establishments both at home and in India. We know but little, after all, of what goes on around us, or of effects produced by modifications of cosmical conditions. Whatever this influence may be, epidemic prevalence does not take place without it. This is so not only in regard to cholera, influenza, dengue and other fevers, where contagion may be questionable, but in the most contagious diseases, such as smallpox and scarlatina, for it is tolerably certain that whatever part contagion may play in etiology, it is of small importance relatively in diffusing disease.

Variation in the atmospheric pressure or moisture, extraordinary stillness of the atmosphere, deficiency in the tension of positive electricity, absence of ozone, fogs, blights, and low forms of life in the air, have all been regarded as predisposing causes. Attention has been

* Aitkin. "The Science and Practice of Medicine."

called more than once to the disappearance of birds from cholera-affected districts at the outset of an outbreak. The dreadful visitation of cholera at Kurachee in 1846, was preceded by days of intense stagnation of atmosphere, and other outbreaks have been preceded or attended by similar phenomena.

It is believed by some that local causes, in addition to certain atmospheric conditions, may determine autogenetic changes in the body which engender disease, and that the existence of a specific primary cause is not always necessary. The general opinion, however, is that an external primary cause, a distinct entity is required; in the case of cholera this is the so-called germ, said to be capable, under favouring conditions, of multiplying to any extent. The advocates of this theory have been energetic in their researches for it among micro-organisms, and have thought that they discovered it in a bacillus. A singular expression of this creed, therapeutically, was witnessed recently in Spain in inoculation for the prevention of cholera; the results, I believe, have not been satisfactory.

There are certain erroneous notions about cholera; *e.g.*, one gives that name to the disease in its fully developed condition alone; but the fact is, that it presents many phases, varying in gravity from simple malaise to collapse, and the coma of the worst forms of fever. Sporadic cholera, or cholera nostras, as it is called, when it occurs in this country, is regarded as a different disease from Asiatic cholera, or cholera maligna; but the cholera of our country is undistinguishable at certain stages from that of India. I believe that the difference in intensity or epidemic prevalence, depends on climate, locality, and the unknown conditions already alluded to. I saw as malignant a case of Algide cholera in the Lambeth Infirmary three years ago, as I have ever seen in Calcutta.

On July 31st, 1884, at the Lambeth Infirmary, I saw a case of cholera with Dr. Lloyd. The man had been an English soldier, formerly in India, ætat thirty-four, well built, but rather slight, an inmate of the workhouse. He was attacked on the night of the 29th of July, with vomiting and purging. He had not been away from the workhouse, and the taskmaster said he had done nothing, nor could he have eaten anything unusual. I found him at 1 p.m., on the 31st, with husky voice, leaden hue of face and hands, corrugated fingers, cramps in the legs, sighing,

eyes half opened, dull, all the symptoms of collapse of cholera, tongue coated and cold, body cold and clammy, temperature 96° ; no urine since admission on the 30th, when he was vomiting and purging frequently; stools were rice-water with flakes; some of the vomit had been kept, it was clear like rice-water, with a sediment like bran. The pulse was faint and quick, the skin not so cold, it is said, as it had been, and the vomiting and purging had ceased for some hours; there was great thirst. Dr. Lloyd's notes of the case are given in a note. No better marked case of cholera than this one could have been seen in Calcutta.

On July 29th, 1884, William Jackson, ætat thirty-five, a porter, who had been an inmate of the Lambeth Workhouse for six weeks previously, and had not gone out of the building during that period, was seized about 3 a.m. with vomiting and purging, accompanied with severe cramp in the legs and abdomen.

About 7.30 was transferred to the adjacent Infirmary, and I found him in a state of collapse: surface very cold and clammy, skin over chest and legs blue, voice (naturally strong) reduced to a whisper, pulse almost imperceptible and very rapid, face pinched, skin over hands wrinkled, and complaining of cramps in abdomen and legs, and intense thirst; feels sick, tongue and breath cold. Temperature 97.

Has been purged about three times within an hour after admission, and about 6 p.m. vomited twice a quantity of dark fluid: no urine.

July 30th, 10 a.m.—Vomited twice during the night, no purging. Temperature 97.4.

6 p.m.—Has vomited several times, purged frequently, very offensive stools, with light coloured flakes; great thirst and restlessness. Temperature 97.6. No urine.

July 31st, 10 a.m.—Quiet night, no vomiting or purging. Temperature 96, no urine passed, thirst not quite so intense.

6 p.m.—Profuse sweating, delirium, no vomiting, purging, or urine. Temperature 96.7.

August 1st, 7 a.m.—No purging, vomiting, or urine. Temperature 97.6.

9 a.m.—Temperature 98, thirst not so intense; sudden change took place about 4 p.m., and death ensued at 6 p.m.

The treatment consisted in giving small pieces of ice to suck, and the administration of warm milk and brandy, and an aromatic mixture with opium to soothe the pain.

Post mortem August 4th, 1884.

Previous History.—The patient had been an inmate of the Lambeth Workhouse since June 24th, 1884, and had not left the building even for an hour during that time. He occupied a ward, in which the other occupants had also been for over a month, and the closest examination failed to detect any of them having been anywhere near the docks, or any possible source of infection. He had been a soldier, and had served in India, where he had suffered from dysentery, but since his return to England, several years ago, had enjoyed good health, and whilst in the workhouse had never required medical treatment, or in any way come under the notice of the medical officer.

Subsequent History.—Two other cases of severe diarrhœa, terminating favourably, broke out in the workhouse, but not from the ward where the deceased came from, and for some time before and after this case of cholera, there appeared to be what usually arises every summer: a number of cases of ordinary diarrhœa, which are successfully treated without removal to the Infirmary.

“There is no such thing,” says Dr. Hutchinson, of the Bengal Medical Service, “as Asiatic cholera, if we mean by Asiatic that the disease is prevalent in India alone, or any given part of the eastern continent. British cholera, Asiatic cholera, Damietta cholera, are all essentially the same disease, though it may be differing in degree and virulence;” and he remarks that cases of cholera appear every year in England in the Registrar General’s returns, which under conditions which appear from time to time would produce an epidemic, as is the case, only more frequently, and with more activity in India, China, Egypt, and elsewhere.

Hutchinson also says that the law under which cholera originates, disseminates and declines is as rigid as any known law, and shows that in 105 outbreaks among European troops in India, and in jails, it was found that the percentage of deaths, which had reached eighty per cent. in the first fourteen days, rapidly diminished. He further remarks that “while three to five days represent the period of incubation of a typical invading cholera, fourteen days mark the limit of an advance of a typical epidemic. This is true of epidemic cholera wherever it appears, whether in Europe or Asia, whether in a city, amongst troops, or in a jail,”* and adds that the period of incubation with regard to individuals is forty-eight hours, deduced from certain well-known cases; but this, I think, is not to be relied on; it may be longer or shorter.

The suddenness and virulence of certain outbreaks are remarkable, and point to some factor apart from contagion or local insanitary conditions. I append the case of Kurra-
chee, and one or two others that illustrate this:

At Kurrachee, in 1846, on Sunday evening, June 14th, there was a sudden change in the atmosphere, the wind veered from south-west to north-east, and a thick lurid cloud darkened the air. Later on in the evening cholera appeared in thirteen corps of the troops stationed there; it increased in violence till the 16th, when 297 cases were

* Hutchinson, “Cholera, its Cause and Mode of Dissemination.”

admitted, of which 186 died, many with frightful rapidity ; after that date it gradually declined, 814 cases and 442 deaths having occurred between the 15th and 18th inclusive.*

“While proceeding up the China sea, in one of the late East India Company’s ships, we were suddenly attacked by cholera, men falling on deck as if struck by lightning. This continued for three days, when the visitation as suddenly ceased. As we were then using the same water that we had been drinking for three months previously, and from the time of leaving England, there could have been no contamination of the water in this instance ; independently of the fact that it was contained in tanks into which extraneous matter could not possibly have entered. A precisely similar outbreak occurred on board H.M.S. ‘Undaunted,’ while proceeding down the China sea. As the cases continued to increase, the surgeon at the end of three days recommended the captain to change the course of the vessel. This was no sooner done than the attacks ceased, not a case occurred afterwards.”†

Fabre and Chailan relate the following :—“The first case of cholera took place at Aix on June 19th, 1835 ; the second case did not occur till July 15th. The sixteenth and the twelfth regiments of the line, numbering 536 men, who occupied the Italian barracks, returned from their exercise at half-past seven in the morning. The soldiers at once went into the various rooms, opened the windows, took off their coats. A gust of burning wind having suddenly penetrated into the barracks, many of these men fell on to their beds as if asphyxiated. The colonel and lieutenant-colonel, acting on the advice of the surgeon-major, mounted to the second floor, and felt themselves the influence which was having such an effect on the soldiers ; one of these superior officers died from cholera within twenty-four hours ; the other and the surgeon-major were very ill for several days. On this very morning twenty-one men of the twelfth regiment of the line were taken to the hospital, and ten others in the after part of the day. Fourteen died in the first twenty-four hours.”‡

“On June 20th, 1845, Dr. Darby wrote from Cawnpore to the Medical Board of Bengal, that during the four pre-

* Bryden. “Cholera in the Bengal Presidency from 1817 to 1872.”

† Parkin, “Are Epidemics Contagious?”

‡ *Journal d'Hygiene*, November 3rd and 17th, 1887.

ceding days the station had been struck by cholera in its most malignant form. Amongst 2,212 Europeans, there were ninety-four cases and sixty-four deaths, whilst among 16,000 natives there were only ten deaths. This epidemic only raged some days and disappeared completely.”*

“In 1884 in London, in the district of Savoy, there were in a few days 537 deaths from cholera; the suddenness of the outbreak was very remarkable. The greatest local diffusion seems to have been reached on the second day, if not on the first. During two days it prevailed with the same intensity, and in the two following days it showed a diminution of fifty per cent.” (Dr. Snow).†

The suddenness of an outbreak may be followed by an equally rapid decline, and the remarkable alternations, whether for better or worse, caused by changes of weather, fall of rain, depression of temperature, thunder-storms and gales of wind, are very suggestive of the influence exerted by meteorology on its progress. Sudden outbreak followed by rapid decline was well illustrated when cholera attacked our troops and ships in the Crimea. It often occurs in India. Let me give you examples, one from my own experience.

After its arrival in the Levant, the French army had suffered a great deal of sickness, but the British army had been comparatively free up to the 19th of July, when cholera appeared among our regiments in Bulgaria, and by August 19th had killed 532 men. Before appearing in our army it had attacked French ships of war in the Mediterranean and their army in Bulgaria, making great ravages among the three divisions marched into the Dobrudja and in the ships. In a day's march, sometimes within the space of a few hours, hundreds of men dropped down in the sudden agonies of cholera; out of these three divisions no less than 10,000 lay dead or struck down by sickness.

The disease appeared in the British fleet, and on the 11th and 12th of August the admirals put out from their anchorage, hoping thus to arrest its progress. It nevertheless raged with a violence rare in Europe; the “*Britannia*” alone lost 105 men, and the number of sick was so great as to render the usual duties impracticable. “The waywardness of the disease on board the British ships was extraordinary; it spared the officers, who partly by kindness

* Bryden. “Cholera in the Bengal Presidency from 1817 to 1872.”

† *Journal d'Hygiene*, loc. cit.

and sympathy, partly by remedies, seemed often able to fight the disease, or make the men think they did so."

Almost suddenly the cholera ceased on board ship, the survivors returned to their duties, all mention of the terrible tragedy was dropped, and in a few days from the time when cholera had been at its height, the crews were ready to embark the troops and land them in the Crimea.*

The Adjutant General had been seized by cholera on Thursday, the 21st of June; he lay in a critical state, though the medical officer entertained strong hope that the remedies would bring on the re-action desired. "Then (on Saturday), however, there broke from a summer sky, not observed to be angered before, the extraordinary thunderstorm of the 23rd of June, carrying with it great torrents of rain; and the swift atmospheric change implied by an outburst so violent extinguished at once every hope of bringing about a re-action." Estcourt died the next morning.*

In 1851 I was ordered to Dacca to take medical charge of the seventy-fourth Native Infantry, which was suffering from fever. I found the regiment—all except two companies which were away on detachment—prostrated. The regimental and other extempore hospitals were full, and there were not enough men left to carry on the routine duties of the station. The fever was malarial intermittent, remittent and typhoid. In a short time I was directed to embark all, invalids and convalescents, on board a fleet of native boats, and take them up the river for change of air. I do not remember the exact numbers, but there could not have been less than from 400 to 500 men. The boats were decked with bamboo and covered with thatch, and held from fifteen to twenty men each, and there were thirty to forty of them. Our orders were [to move up the stream a few miles every day, and make fast to the bank at night. Our mode of progress when there was no wind was that of tracking. There was no cholera in the regiment or in the station, that I know of, when we started. The men were prostrated with fever, many still suffering from it, and some had splenic or other visceral complications.

We got on well for three or four days. It was the cold season; the change appeared to be doing good, and some of

* Kinglake. "Invasion of the Crimea," vol. viii.

the men seemed to brighten up, but none of them liked the move. We made fast to the banks every night, when the men, who were able to do so, landed to cook their food; they were chiefly Hindoos of high caste. The river flowed through a flat alluvial country; the banks, which were but a few feet above the river, were of sand, and the land beyond was covered with light vegetation. There were no inhabitants near at hand, and I do not remember seeing any but an occasional villager, except the few who passed connected with other boats.

We had been out a few days, moving daily up the river, when one morning it was reported that a boatman had died of cholera very rapidly in the night. That day more cases occurred, the sepoy became affected, and cholera at once invaded the whole fleet with great virulence. It was most distressing to see the poor creatures in the last tortures of cramp, vomiting and purging. We did all we could, but it was of little avail. We moved on daily, as our orders to do so were stringent, but nevertheless the disease continued. We were in the open country, on a magnificent river; the weather was fine, the temperature pleasant, and, but for our floating plague-boats, all looked bright and cheerful. The men were in agonies of despair, and entreated to be taken back to head quarters, as this, they said, was killing them. Each day produced its fresh list of cases and deaths; these soon became so numerous that the bodies were committed to the river without ceremony. It was remarkable how the days differed; on some the disease appeared to be aggravated; those who were ill got rapidly worse and died, and more fresh cases occurred, often fatal in a few hours; whilst on others the very reverse would take place.

I repeatedly urged the officer in command to return, but he could not do so without orders. After some days, when it appeared that we were going to lose all our men, we held a council and determined to return. We did so, and, be the explanation what it may, the disease ceased, and by the time we got back to Dacca it had disappeared. A large proportion of those we brought back had to be invalided. I may add that I made careful enquiry day by day if cholera had occurred anywhere in our proximity, but heard of none. It was not a cholera season. I had discussed this aspect of the question and a possible outbreak with the P.M.O. before we started.

There is room for speculation as regards the causation of the sudden outbreak of the disease, its varying intensity on different days under apparently similar circumstances, and its rapid decline and cessation as we returned. The landing in the evening and lying by the bank all night were indicated as being mainly concerned, but this was done by other boats, and we heard of no cholera in them. The state of health of the men—all suffering from malarial fever—must be borne in mind, and the question of this as a cause may fairly be entertained. The country we passed through was open and healthy; the food and water were such as Hindoos approve, and I may say the disease was not confined to the Hindoos, for there were Mahomedans among the Sepoys as well as among the boatmen. However, it is not with the view of offering any explanation of the etiology of this outbreak that I have detailed it, but merely as an example of the varying phenomena which may be met with in a cholera outbreak, and the rapidity with which it may cease.

The following is an example of the benefit of change of locality in an outbreak of cholera. In 1855 H.M. 52nd Foot were stationed at Lucknow, in a set of large buildings which had formerly been used as the royal stables. A sudden and severe outbreak of cholera took place amongst them, which, causing great mortality, produced much depression amongst the men. A committee of medical officers was assembled, which recommended immediate removal to camp outside the city on the Cawnpore Road. Notwithstanding the great heat and the consequent danger of sunstroke, of which there were indeed a few cases, the cholera entirely ceased and the regiment was restored to its original state of health.

Locality, apart from insanitary conditions, its position and physical characters are to be taken account of. Elevation has an influence, though less positive than relative, but cholera has occurred at Simla and other hill stations in India over 7,000 ft. above the sea.

The nature of the soil and the geological characters of a district have probably something to say in the localisation of cholera. Some have thought that it is less prevalent on sandy, porous ground, on granite, metamorphic and trap rocks, on laterite and volcanic formations and on the primary geological deposits, but the wide-spread distribution of the disease does not point to this as a very im-

portant factor. Cholera prevails in deltas, but that it may occur with great virulence even in a desert we know from Indian experience, and Sir Thomas Seaton's account of his march across the desert of Pat in Sind, proves not only that it may occur, but it suggests its relation to fever and insolation, a point I shall have to notice later.

On May 3rd, 1839, a convoy of over 4,000 camels, escorted by two troops of irregular cavalry, a wing of the 23rd Bombay Native Infantry, a wing of the 42nd Bengal Native Infantry, a company of one of the Shah's regiments, and a troop of irregular cavalry, started from Shikarpur to join the army in Afghanistan. There were also a number of convalescents and a multitude of camp followers. Their road lay across the desert of Pat in Sind, which begins thirty miles west of Shikarpur, and stretches to the foot of the Bolan Pass. They reached Rojhan on the borders of the desert on May 28th, after encountering many difficulties. On May 29th they started across the desert, and from the very beginning suffered severely from want of water. Deaths occurred during the 31st, but it was not till the evening of that day that cholera appeared. They were obliged to make a *detour* to search for water, and the extra fatigue added much to the sufferings caused by absence of water, by the extreme heat, which rose in the tents to 119° , by the fierce desert wind and the myriads of flies. Their route was marked by scores of men ill and dying from fever, cholera and sheer exhaustion. "Some of the sufferers were fast sinking from fever, and were delirious; others appeared to be just seized with cholera; many exhausted by thirst, and overcome with fatigue, were bitterly bewailing their sad fate." Cholera, fever and sunstroke worked great devastation, and on June 3rd the desert wind began to blow with increased violence. "Some of the men sank at once as if struck by some poisonous air, others were brought in alive, but dying fast—quite shrivelled in appearance, as if the hot wind had dried up all the juices in the body." Officers, as well as men, suffered severely. "The scene in Major L——'s tent I shall never forget; it was appalling. B——, suffering all the agonies of cholera, was the colour of lead; H—— was raving; S—— and M——, both of them speechless and helpless from utter exhaustion, appeared likewise as if struck with cholera."

The march across the desert occupied between seven

and eight days, and these sufferings continued the whole time, disease not beginning to diminish till they reached Baugh on the morning of June 6th.*

The greatest intensity of cholera incidence is not always found to be in the most populous places. "It was among the wandering tribes of the desert of Arabia, and among the scattered population of the mountainous region of the Caucasus, that cholera, on its first invasion of these countries, prevailed in its greatest intensity, and committed its greatest ravages. In Arabia, a third of the inhabitants, according to Moreau de Jonnes, perished, while in the Caucasus, 16,000 or two-thirds of the population were attacked, and 10,000 or nearly half, died. During the outbreak of cholera in Jamaica in 1850, at Kingston with a population of 40,000, not more than a sixth, or sixteen per cent. of the inhabitants were cut off. But at Falmouth, a small town, the deaths amounted to a third. In Port Maria, a still smaller town, two-thirds of the population, or 600 out of 900 perished. 'At first,' writes the Rev. T. Simpson, 'the epidemic was mild in its type, and yielded readily, in most cases, to the treatment of our medical men. But, on the 1st of December, it burst on the town like a flood, carrying off 400, nearly half the population, in the short space of ten days.' In the small towns and villages the mortality was much greater."†

Bryden says "The geographical distribution of an invading cholera is purely a phenomenon of meteorological significance. Epidemic cholera is never in any case spread *over a definite geographical area* by human intercourse alone; nor can human agency cause the boundaries of a natural province which has been occupied by cholera to be transgressed, so that a cholera *epidemic from such a source* shall appear in the province immediately adjoining and become generally diffused among its inhabitants.‡

Seasonal prevalence in India varies according to the district. Generally speaking the minimum intensity is in the winter months, while the maximum varies, falling sometimes in the summer, sometimes in the spring. In the endemic area and Madras there are two maximums, in the former in the spring and winter months, in the latter in the summer and winter months. Outside India the

* Major-General Sir T. Seaton, K.C.B. "From Cadet to Colonel."

† Parkin. "Are Epidemics Contagious?"

‡ Bryden, "A Report on the Cholera of 1866-68."

maximum is generally in the autumn and winter months, and we have Pettenkoffer's authority for stating that in Prussia the minimum is in March and April, the maximum in September, the rise from July being rather rapid. This question, as well as many others, such as the caprice of an epidemic shewn by its passing over many places in an area attacked, and its varying intensity in different years, is as yet unexplained.

Statistics given by Dr. H. W. Bellew, C.S.I., in his "History of Cholera in India," shew that a definite and fixed relation exists between cholera prevalence, and seasonal distribution of rainfall and the condition of the soil which receives it; drought followed by irregular scanty rainfall, scanty falls followed by heavier ones, and *vice versa*, are all favourable to cholera prevalence; that intensity of rainfall phenomena and cholera activity have a marked tendency to run in three years' cycles, greatest intensity being in the first year, followed by gradual diminution; dear food or famine distress influences the severity of an epidemic. Statistics are given for the years 1862-81, and in each cycle cholera followed the course laid down—except in that of 1875-77, when instead of abating it increased;—in each year of that cycle there was drought in the previous year, followed by excessive monsoon rains, aided by famine.*

The conditions of the subsoil water, its fluctuating level and its stagnation, are no doubt concerned in the development of cholera, as beyond a doubt they are in that of fever, of which it is a potent factor; for it is certain that a water-logged subsoil and undrained ground, materially affect the public health and add to the mortality from fever, and probably also from cholera.

With regard to epidemicity in the endemic area, Cunningham says "In all parts of the country there is a most marked difference between the results of different years. In some years the disease is in abeyance, in others it is epidemic, and between these extremes there are many gradations. Even in the endemic districts, the difference between an epidemic and a non-epidemic year is very striking. In Nuddea, for example, in 1871, only 528 deaths from cholera were registered, in 1882 the number was 11,020. In Backergunge in 1871 the number was 291,

* W. H. Bellew. "History of Cholera in India."

in 1877 it was 19,177. Similar results are to be seen in the districts outside the endemic area."

"It is not to be supposed from the above remarks that the periods of cholera abeyance and cholera prevalence occur simultaneously all over the country. The case is rather the reverse. In a year when one province is suffering, another may be enjoying remarkable immunity. It does, however, usually happen that marked cholera abeyance or cholera prevalence is observable over large areas—areas which often include many districts. In some years, as notably in 1874, there was a marked abeyance of cholera over the greater part of India. In the endemic area and in the districts lying around this area, cholera, as a rule, occurs rather in a large number of individual cases here and there than in epidemic outbursts."*

Since 1877 records have been kept of the attendants on cholera patients in military and jail hospitals throughout India. It is found that 5,696 cases occupied 10,599 attendants, and that only 201 of these were attacked, or 1.9 per cent. The same immunity of attendants is shewn by the statistics of London Hospitals in 1866; and in the General and Medical College Hospitals of Calcutta, where cholera cases are admitted indiscriminately with others, the disease has never spread; but this, indeed, has been the experience in India generally.

With regard to the spread of cholera, theories of contagion and diffusion by human intercourse do not explain the movements of epidemics, for the history of the last fifty years shews that though means of communication have greatly multiplied in India, as everywhere else, epidemics have neither increased in frequency, progressed more rapidly, nor altered as to their general direction. In fact, of places that lie on the main line of traffic, some suffer least, while others, more inaccessible, suffer most.*

With reference to dissemination, it has been asserted that cholera breaking out in such an assembly as the Hurdwar Fair, on the dispersion of the pilgrims the disease has been diffused in all directions over the country; but, on careful analysis of facts, it will be found that although the pilgrims on the spot have died in all directions whither they have travelled, that cholera has appeared in others only in the

* "Cholera—What can the State do to prevent it?" Cunningham.

direction in which the epidemic was moving. Further, it has been found in reported cases of importation of cholera from one station to another, that the disease had already manifested itself in the district, before the particular case which was supposed to have imported it had arrived. Wherever thorough investigation has been possible, it has been found that explanation, based on the theory of contagion, fails to account for facts.

It is certain that the most frequented routes of human traffic, or the most direct lines of intercourse are not always marked by frequency or intensity of cholera, and it seems especially remarkable, if cholera be spread by human intercourse, that since the opening of the Red Sea route in 1842, and the Suez Canal in 1869, the disease should not have been conveyed to Europe by the stream of vessels which are daily sailing from Calcutta, and other cholera localities.

Cholera seems to have an affinity for certain districts—even streets and houses. I remember several houses, or groups of houses, in Calcutta, which were known to be liable to suffer from cholera, and it is so still in a marked degree, as shown by the last report of Dr. Simpson, the very able health officer of Calcutta. One side of a street may suffer, while another escapes; a small stream may divide a cholera affected district from one perfectly free. It is worthy of notice, also, that certain trades, such as the tanners, are said to confer a prophylactic influence.

During epidemic prevalence cholera never attacks all the places in the area over which it is diffused, sometimes leaping over places in the direct line of its course and returning to them later, during the same epidemic. It is a remarkable fact also, that in Bengal an epidemic always moves upwards,* not necessarily along the great lines of traffic, or with the rivers, but rather against them. Frequently places attacked at the same time are widely distant, and this is constantly observed in Indian epidemics, only a comparatively small proportion of villages and towns being attacked in any large area where an epidemic, however intense, prevails.

The apparent caprice and fluctuation of a cholera epidemic are shown by the following extract from the "Report of the sanitary Commissioner for the the Hyderabad Assigned Districts for 1884 :"—

"The mortality from cholera in these districts varies

* Cuningham. "Cholera—What can the State do to prevent it?"

greatly in different years, *e.g.*, 87 deaths in 1884 were preceded by 27,897 in 1883, and it will be seen on comparing the returns since 1869, that a sudden fall like the one mentioned has happened two or three times, and that in only two instances (1870-71 and 1881-82) have the returns for two consecutive years been almost equal."

The following table shews how cholera varies in its incidence from year to year, and relates to the time when I was with the Prince of Wales in India.

						Cholera deaths.	
						1875	1874.
Bengal Proper and Assam	116,606	73,354
North West Provinces	41,106	6,396
Oudh	23,321	68
Punjab	6,246	78
Central Provinces	14,643	14
Berar...	22,465	2
British Burmah	761	960
Madras and Mysore	97,051	313
Bombay	47,573	37
Rajpootana, Hyderabad and Central India	14,649	4

These variations in intensity occur everywhere in India, and can hardly be explained by the theory of contagion; we know this much, however, that bad sanitation, especially impure water, invites cholera and increases its severity, while a good sanitary state tends to prevent it, or to lessen the intensity of the epidemic. This was shown in the case of Spain in 1885, where the great cholera outbreak was undoubtedly connected with sanitary negligence.

Etiology.—There is much in the symptoms and general conditions of cholera, to support the view which has been advanced by several observers, that it is only another form of fever, and that it owes its origin to analogous causes. Certainly fever and cholera frequently prevail at the same time, and have so much in common that it is difficult to differentiate between them, especially during epidemic prevalence.

In an outbreak at Umritsar in 1881, Dr. Ross says:—"Fever in the city did not appear in an epidemic form until September. It was preceded by cholera early in August of an extremely fatal type. This later on, when masked by fever, was difficult to recognise." Of another outbreak he says:—"In Kohat in 1869, an outbreak of

fever very similar to the Umritsar epidemic, followed by cholera, occurred. It was then observed that it was often impossible to differentiate them.

Dr. Chevers expresses similar views, and refers to a series of illustrative cases which show how closely cholera and malarial fever are etiologically allied. Some indeed extend the community of origin to other diseases, such as insolation, dysentery, influenza.

The following is a remarkable instance of the community of origin, if not identity of cause which occurred under my own observation, during the Burmese war in 1852. A party of European troops had been encamped for certain strategic purposes on some ground which had been recently cleared of dense jungle. They were rapidly attacked by fever and dysentery of the worst type, and, I believe, by cholera too. Many cases of fever and dysentery were sent to the Field Hospital at Rangoon under my charge. The fevers were remittent, of the most fatal type. The dysentery was equally fatal, the symptoms most severe, rapidly passing into a state of collapse, and after death the large intestine found to be gangrenous almost from end to end. There were other cases of less severity, but the intensity of the morbid agency, and its power of inducing different pathological conditions was well illustrated in the cases referred to.

The sweating sickness of Mahwar, described by Dr. Murray in 1840, was probably only another manifestation of pathological conditions originating in the same cause to which cholera may be referred, and abundant illustration of this might be advanced did time permit. The different forms in which the morbid agency, whatever it be, manifests itself are the result presumably of an evolutionary process determined by constitutional predisposition and on certain conditions of the surroundings, of which we know but little.

The type of cholera varies considerably in different epidemics; vomiting, purging, cramps, early and late appearance of collapse, consecutive fever, &c., present great difference in the modes in which they occur, whilst the fatality, also, of some epidemics is much less than that of others; there can be little doubt that these characters of an outbreak are influenced by meteorological and local causes.

Aitken says :—"It is desirable if possible to get rid of

the term *cause* as applicable to any particular disease. . . . There is no disease I know of which acknowledges any single cause." But rather says he, "Ought it to be our business to find out the many and ever varying factors or conditions which as antecedents combine to produce disease, and while we must acknowledge the influence of many agents in aiding and abetting these factors, we must mainly look to the physiological agencies within our own bodies during life as competent to bring about many forms of disease,"* and this may be applied to the question of the causation of cholera.

Chevers says: "The discovery of the cause of cholera will probably never be vouchsafed to a man of narrow and one-sided views. I believe that nothing valid will be revealed to us, unless we grasp and correlate all facts."† We may not know the cause, but it is assumed by some, that, though we have never seen it, there is a specific organic germ, which being introduced from without, gives rise to the disease. I venture to think that even this is not yet proved.

Chevers says: "I have never seen or heard anything which, upon close investigation, shakes my firm impression that a specific poison is not contained in the stools."†

There are several theories of the causation of cholera; briefly, they are as follows:—

That it is due to a miasmatic poison, which being absorbed by the lungs or alimentary canal, produces a primary disease of the blood, where it is rapidly multiplied, and causes disturbance of vital functions; that the diffusion of the disease is effected by human agency, the specific poison being carried by the persons and effects of those who have been exposed to it.

That it is due to a specific poison or germ which passes from the bowels of one person to those of another, chiefly by water, the poison being contained in the dejecta.

A modification of this theory assumes that to produce cholera, the organic germ must be in a certain vibrionic stage of decomposition. This germ may be preserved in a dry state for years, but whether fresh or old, it undergoes rapid changes in water. Oxidisation, acids and certain degrees of temperature, it is inferred, can render it harmless.

* Aitken. "Animal Alkaloids."

† Chevers. "Cholera Asiatica Maligna."

According to Pettenkofer, a germ is developed in a damp porous soil with fluctuating subsoil water level, impregnated with organic matter—it is, in short, earth-born. The germ must remain in the soil some time and ferment before it acquires poisonous characters; it then rises into the air as a miasm, and thence effects entry into the body by means of air, food or water. The germs further developed and multiplied are again expelled. In considering the effects of traffic on the transmission of cholera, he says: “The dejecta are not the only means of spreading cholera, and possibly in that way they are harmless.” The conditions above stated, combined with personal susceptibility, must concur for the production of an epidemic.

In 1883, Professor Koch, after investigating cholera in Egypt and later in India, discovered a bacillus in the alvine discharges of cholera patients, which was announced to be the germ which caused the disease. The doctrine of contagion received thereby an impulse by which the dread of it became enhanced, and southern Europe for a time was almost demoralised by fear, whilst the old measures of coercion and quarantine threatened to be reimposed with greater severity than ever.

In May, 1884, the Secretary of State for India, at my instance, despatched a commission (Drs. Klein and Gibbs) to investigate the subject in India. In March, 1885, they submitted their report, and a committee of physicians and pathologists was convened to consider it. The following conclusion was arrived at:—that comma-shaped bacilli are usually found in the dejecta of persons suffering from cholera, but that there are no grounds for assuming that they are the cause of the disease; that they are, in fact, but epiphenomena—thus confirming the conclusions of Lewis and Cunningham, arrived at years before, after a long and careful microscopic study of the disease in India.

Aitkin in his work on “Evolution in its application to Pathology,” remarks:—“Perhaps the brilliant success which has been achieved by the recent studies of disease producing organisms or other materials acting on us from without—a success not equalled in any other field of medical enquiry—has made some think too little of those changes within ourselves, which occur in such ordinary conditions of life that they may be called spontaneous, yet these are not less important in the production of diseases, and must be studied, just as in agriculture, soil

must be studied as well as the seeds.”* I venture to think the above suggests the danger of too hasty generalisation, and of reasoning on insufficient data.

Whilst fully recognising the great value of these bacteriological researches and their bearing upon etiology, the full importance of which cannot yet be estimated, I demur to a microbe being accepted as the solution of such a problem as the cause of cholera.

Dr. Bryden, whose unrivalled opportunities of studying cholera in the most exhaustive manner give great weight to his opinion, maintained that cholera is due to a miasm and has a perennial abode in certain areas of India, and in other districts is renewed by invasion from these areas. That the cholera germ or miasm is earth-born and aërially conveyed, and that the disease has no power of continuous manifestation throughout the year. He thought it could be transmitted by fomites, but that the aggregate of cases so transmitted would not produce an epidemic. He thought the presence of the cholera miasm, a humid atmosphere, and certain prevailing winds essential to the production of an epidemic, and that its duration bears some relation to the humidity of the locality. Re-appearance after invasion and outbreaks are governed by the same laws as invasion.

Another theory assigns to cholera a cause independent of a specific germ. Dr. E. Goodeve says, “May it not be a mistake to consider this as a simple body either generated from without, and air-wafted to a particular spot, and then multiplying itself indefinitely, or as a locally generated agent, and spreading over certain areas? Might it not be more in accordance with facts to suppose that neither a miasm from without, nor a miasm from within exclusively contains the specific poison? Might it not be that two factors are needed, the one some air-borne material, or some *dynamic modification* of atmospheric elements coming from without; the other some local element, neither being potent unless united? The peculiar atmosphere sweeps along hither and thither, and it is only when it meets with the other peculiar substance that the poison is generated or the effect produced.”†

* Aitken. “Evolution in its application to Pathology.”

† Reynold’s “System of Medicine.” Article on Cholera, by E. Goodeve.

For my own part I am unable to convince myself that any of these theories satisfactorily or conclusively explain all the phenomena exhibited by a cholera epidemic, or that one view can be accepted to the absolute exclusion of all others, for there is much to support each. Whether this ultimate cause be a bacillus, a chemical molecule, or the outcome of forces surrounding us, of external influences acting on cerebro-spinal centres and producing certain perturbations of physiological processes, or perhaps developing an autogenetic poison, is a question still demanding solution, and I agree with Chevers that the cause will probably not be revealed to anyone who searches with narrowed views. There is a great tendency in these days to trace all disease to a specific exterior cause, but we must not lose sight of the possibility of poisons autogenetically developed with which the researches of Gautier, Peter, Brown, Lauder Brunton and others are making us familiar, or of altered conditions of innervation, deranged natural physiological processes of vaso-motor action caused by forces acting from without, giving rise to disease. The primary cause, *i.e.*, the factor or group of factors which cause cholera, is still unknown, but so much, however, has been learnt of its habits, that in Europe and India we have come to know that action based on any theory of contagion is as useless as it is unprofitable. As to the local conditions which foster and develop, if they do not cause cholera, the most potent and protective safeguards against them are cleanliness, pure air, pure water, good food, clothing, lodging and healthy conditions of living; and, with reference to water, as Dr. Simpson of Calcutta remarks, "A study of the distribution, progress and seasonal changes indicates that the chief factor is a want of pure water."*

Happily, however much we in England and India may happen to differ as to certain points in the etiology, we are in accord as to the principles on which preventive sanitary measures should be conducted. As regards coercive measures, such as cordons and quarantine, they are rejected as useless, bringing many evils without preventing the spread of disease.

We have been charged by other nations with maintaining these views in accordance with the commercial interests of

* "The Progress and Distribution of Cholera Mortality in Calcutta," W. J. Simpson, M.D.(Aberd.), D.P.H., Camb.

our country, but on what grounds it is difficult to understand, for, as Chevers says, "Being quite unaware what that interest is, save that it appears to me that if I were a Bristol merchant it would not be to my interest to see that port impested with cholera. . . . I remain absolutely unconvinced of the protective efficiency of sanitary cordons and quarantine in cutting off the approach of that which does not travel, and in arresting the propagation of that which is never propagated." *

The belief in transmission by human intercourse is still firmly held by the highest authorities ; few consider that there is danger from mere contact or personal communication, but that the danger lies in the transmission of the germ through water or other channel from the bowel of one person to that of another ; hence they properly insist on what others equally admit—the importance of the purity of drinking water ; who do so not because it contains a germ, but because impurity tends to develop the pathological conditions which result in cholera. For my part I am unable to accept the water theory as a sufficient explanation of all cholera outbreaks, especially in those which occur where the water is beyond suspicion of cholera contamination, and my agnosticism leads me to seek the explanation in causes of a wider and more general character, though I desire to speak as one who is still waiting for further information, and who, though strongly impressed with the incommunicability of cholera by the ordinary modes of contagion, is still not prepared to assert dogmatically that under certain conditions it may not become communicable by some miasm engendered in localities such as quarantine lazarettes where disease is intensified by crowding. I hold, moreover, that until contagion in any form be entirely disproved, authorities are justified in adopting measures which, like those in force in our own country, whilst avoiding all oppressive or coercive interference with personal liberty, take reasonable precautions against possible sources of infection and give full effect to all known practical measures against the importation or diffusion of disease.

Coercive Measures and their Results.—The evil results of the contagion theory, as interpreted in other countries, have been shown not only in the rigours and hardships of quarantine, whereby great suffering and incalculable damage to commercial interests have been effected, but in the general

* Chevers. "Cholera Asiatica Maligna."

panic and demoralisation which have degraded and deranged society generally.

The state of Southern Europe during the recent cholera was pitiable, and the measures for fumigation, isolation, and interference with personal liberty would have been ridiculous had they not been so mischievous. The following notice, extracted from a daily paper of the 27th of August, 1887 (*Scottish News*), reminds one of the state of feeling in the Middle Ages, when the Jews were victimised as the supposed originators of the plague:—

“*The Cholera in Sicily—Sanguinary Scenes.*—A letter from Palermo, published in Vienna, reveals a startling state of things in Sicily, consequent upon the reappearance of the cholera. The ignorant population attribute the outbreak of the terrible epidemic to the evil disposition of the Government. Assassination, incendiarism, and sanguinary encounters with the gendarmes and troops are reported from different parts of the island. The measures taken by the authorities since the last visitation of cholera, such as the disinfection of certain villages, suppressing unwholesome wells, and reinforcing the medical staffs, have been misconstrued and taken by the people as a sure indication that the Government wanted to send them the disease. Special precautions were taken at certain places, and shortly afterwards a case of cholera occurred at one of them, the patient being transferred to the cholera hospital recently erected. The same night a band of villagers armed to the teeth, set fire to the building, and murdered the sick man, whom they accused of being paid by the Government to spread the malady amongst them. They then repaired to the high road, and, taking up a position behind the bushes on either side, they there awaited the arrival of the gendarmes, whom the mayor had sent for when the first alarm reached him. When the gendarmes came up with the miscreants they were greeted by a deadly fusillade that cost the life of their brigadier. The aggressors fled to the neighbouring woods, where they were attacked the next day by the troops. Half of them were shot and the others taken prisoners, but not before many soldiers had fallen. At Leonforte the armed inhabitants had a formal encounter with the carabinieri. Dispersed after a savage combat, the bulk of them fled to the monastery of San Vincenzo, where they barricaded themselves and underwent a regular siege. The carabinieri, reinforced by infantry, burst open the doors, and forced their way into the monastery. After a desperate

resistance the besieged were overpowered, and the survivors were marched off to prison under a strong escort. A state of siege has been proclaimed in the town. Similar events have taken place at Caltagirone. Seventy-eight peasants have been arrested at Catania. The island seems to be in a complete state of revolution."

A similar feeling exists in other parts of the world. Take the following absurd instance from the *Times* of the 22nd of January, 1886: "Two Japanese sailors died from cholera during the short journey from Kobe to Nagasaki. Their bodies were thrown overboard. The Japanese authorities immediately forbade fishing all along the coast.—*Sanitary Record*."

It is satisfactory to know that a modification of coercive measures has taken place in Southern Europe during the recent manifestations of cholera. Whether this be the result of the conviction, forced upon the people by events, of the futility of such proceedings, or whether it may be in some measure the result of the emphatic declarations made against quarantine by the British and Indian delegates at the Roman conference, I cannot say; but we hail even this much as an augury of better things to come, and regard it as an indication that methods worthy of the dark ages will be discarded as they have been in Britain and India—I wish I could say in our colonies!

In Britain we have the moderate but more effective system of prevention laid down by our Local Board. In India, where a sanitary service has been organized for more than a quarter of a century, the policy of the government, taught by experience, rejects all theories of causation and propagation as a basis for sanitary work, for they have learnt that any attempt to carry the doctrine of contagion into practice has no good results, but is productive of harm, for it involves oppression, and aggravates the evils it is intended to prevent. Coercive measures have been discarded, reliance being placed on sanitary measures alone, and the results seem to be satisfactory, judging from the following statistics which are taken from the 21st and 22nd Annual Reports of the sanitary commissioner with the Government of India.

DEATH-RATE PER 1,000 FROM CHOLERA.

<i>British Army</i> , 1860-69.	1870-79.	1880-83.	1884.	1885.
Bengal ... 9.24 ... 4.18 ... 2.49 ... 1.34 ... 1.17				
Madras ... 2.56 ... 1.68 ... 0.90 ... 0.93 ... 0.19				
Bombay ... 4.80 ... 1.53 ... 0.45 ... 4.85 ... 6.92				

Jail Population.

1859-67	10.67
1868-76	3.28
1877-83	3.61
1884	1.43
1885	3.44

The mortality of cholera is high when it has reached the condition of collapse, or consecutive fever. At the outset of an epidemic probably half or more than half of those affected die. The fatality decreases as time goes on, and this has led the inexperienced to think that they have found some more effective mode of treatment than hitherto known. This diminution in intensity and fatality as an epidemic progresses is not peculiar to cholera epidemics; it occurs in others and was observed by Defoe in regard to the plague in London, during the seventeenth century. In an outbreak of cholera at Kurachee of the first 100 admitted 79 died; of the second, 66; of the third, 50; of the fourth, 40; at a later period the mortality diminished and the cases were less severe.

The following tables show the mortality from cholera in India, during a series of years, and it will be seen that it is a trifle compared with that of fevers:—

MORTALITY FROM CHOLERA IN INDIA.*†
(Including Army and Jail population.)

YEAR.	TOTAL MORTALITY.	RATE PER 1,000.
1876	486,704	2. 47.
1877	637,096	3. 49
1878	319,503	1.704
1879	271,094	1. 45
1880	119,182	. 63
1881	162,290	. 85
1882	351,422	1. 76
1883	249,248	1. 24
1884	287,926	1. 45
1885	386,546	1. 95

* Excluding Calcutta.

† Reports of the Sanitary Commissioner with the Government of India.

MORTALITY AMONG THE GENERAL POPULATION IN INDIA.†

YEAR.	RATE PER 1,000.			
	FEVERS.	BOWEL COMPLAINTS.	CHOLERA.	SMALL-POX.
1876	11·49	1· 52	2· 47	· 53
1877	13·85	2· 15	3· 49	1·009
1878	17·35	2· 22	1·703	1· 64
1879	19·04	1· 35	1· 43	1· 04
1880	14·68	1· 25	· 63	· 37
1881	16·83	1· 37	· 85	· 39
1882	15·75	1· 41	1· 76	· 42
1883	14·37	1·306	1· 24	1· 16
1884	16·72	1· 39	1· 45	1· 68
1885	17·18	1· 48	1· 95	·408

It may be well here to refer to cholera on board ship. It has frequently broken out in vessels in the harbours of affected ports, but has disappeared soon after the ship has gone to sea; in passenger, emigrant and troopships, it makes its appearance from time to time, within certain periods after leaving the port,—varying from two or three days to as many weeks. But, as the people on board have been exposed to the influence of cholera before they left, we may assume that cholera was latent in them when they left.

In some cases, where the port of embarkation was not affected, though the passengers came from a cholera-affected district, and the disease attacked the crew also, it is to be remembered that the ship started from a country in which the epidemic influence was present, though not ostensibly so in the port of embarkation.

Ship-cholera seems to give some support to the doctrine of contagion, but the truth most probably will be found to lie in the fact that the individuals attacked were cholerised before they left the country, and that insanitary local causes on board the ship developed that which was dormant in the

† Reports of the Sanitary Commissioner with the Government of India.

individuals, or that the ship passed through a zone of epidemic influence.

Dr. Sutherland writes:—"The ship or the men must have been in a cholera locality. The men become choleraised, so to speak, and whether the disease lies dormant or shows itself, depends on other conditions being superadded. It would be another thing if cases such as these introduced an epidemic into a perfectly uncholeraised country. But this has never happened; the *aura* must be there before the ships. We cannot tell yet what choleraisation is. We are seeking to know. But we do know that it is set up indigenously and without external importation."

He adds:—"1. A ship lying in an epidemic port may become part of the epidemic port after it has sailed, provided there be men on board who have also been in the locality. 2. A ship sailing on the free open sea may encounter a travelling epidemic and be struck thereby. This has happened in the Bay of Bengal and elsewhere, in the face of the Monsoon." For example, in November 1848, two ships, the "Swanton" and the "New York," were struck with cholera in the Atlantic Ocean, the former twenty-six days after leaving port, the latter sixteen days. Both these vessels sailed from Havre at a time when cholera was prevalent in Germany, but had not reached the west of France. "3. An epidemic may outstrip a steam ship, as happened at Malta in 1865. 4. No cholera-struck ship ever landed an epidemic. 5. What is called the incubation period of cholera is not fixed but variable, and may require nothing but change of temperature to develop it."

Precautionary measures, general and special, against cholera.—The belief is maintained by foreign powers that epidemic diseases, and especially cholera, can be arrested in their progress and debarred from entering into a country by quarantine. This once meant seclusion and isolation for a period of forty days, of persons either affected by disease, or coming from a locality where it prevailed, and is based upon the assumption that the disease is communicable from person to person, either by means of the individual himself, or of his effects. Of late years, the period of isolation has been diminished, even by those who hold the doctrine of contagion.

It is needless to dilate minutely on the evils that resulted from this grave interference with personal liberty;

suffice it to say that they comprised discomforts and horrors arising from the accumulation of people in lazarettes, whereby great inconvenience and personal suffering were inflicted, with hindrance to commerce and the creation of foci of disease, forming an accumulation of evils greater than that they were intended to avert.

Still, could it be shown that by such measures, the propagation and diffusion of disease from nation to nation can be averted, their adoption, under proper management, and with precautions for the personal safety and comfort of those concerned, would be justified as the minor evil. But, if it be true that the diffusion of epidemic disease is dependent in a great measure on atmospheric or general causes, then the futility of quarantine is obvious.

The British and Indian Governments, basing their measures for protection on ascertained facts, and not upon theories, have discontinued quarantine, whether by land or sea, relying upon sanitation and medical inspection, as the only and sufficient means of safety.

The British Government Local Board, recognising the contagious nature of some diseases and its probability in others, has adopted measures of inspection and isolation of the sick, together with disinfection, and purification of ships, effects and persons, insisting at the same time on all that conduces to the establishment of healthy conditions of living, but avoiding undue interference with personal liberty. The following is an epitome of their measures as regards cholera :—

Ships known or suspected to have cholera on board are to be detained by the Custom House Officers until the Medical Officer of Health shall have inspected them.

Those on board suffering from cholera are, if possible, to be moved to a hospital, but if they remain on board they are to be isolated, and all that comes from them disinfected.

Those not suffering from cholera, though coming from an affected ship, are to be allowed to proceed to their destination, notice being given to the Health Officer of the district to which they proceed.

The ship itself and the effects of any on board, who have suffered from cholera, are to be disinfected, and no further detention is to be imposed.

In India, quarantine, cordons and interference with personal liberty, including isolation of the sick, have been discarded as practically useless, attention being concen-

trated upon sanitary measures as the best means of preventing the diffusion of the disease.

The following is a summary of regulations for the army, which, as far as possible, are applied to the population generally.

In anticipation of an outbreak, personal cleanliness is enjoined, the utmost attention is to be given to the sanitary condition of the station; overcrowding is to be avoided and great care to be taken in watching and checking premonitory symptoms.

On the appearance of cholera, bodies of men are to be *at once removed from the affected locality*; great attention is to be paid to the purity of the water supply, and to the nature of the camping ground; all dejecta are to be buried in trenches dug for the purpose.

Purification and fumigation are to be resorted to, both of the room or building in which any case of cholera has occurred, and of the effects of the sufferers.

Temporary buildings are to be erected as hospitals, but, in the case of the general population, removal of the sick from their homes is not enforced. It is pointed out that no danger is incurred by attending on the sick.

Dr. Southwood Smith says, "the object of quarantine is to prevent the introduction of epidemic disease from one country into another," and the whole machinery of it is based on the assumption that by an absolute interdiction of communication with the sick, or infected articles, the introduction of epidemic diseases into a country can be prevented.

This assumption, however, overlooks the presence of an "epidemic atmosphere," without which it is now by many contended that no disease will spread epidemically. "Allowing therefore to contagion all the influence which anyone supposes it to possess, and to quarantine all the control which it claims," there remains this primary and essential condition which it cannot reach.

Experience shews that "the influence of an epidemic atmosphere may exist over thousands of square miles, and yet affect only particular localities." Why does it so localize itself? Probably because it finds there certain local or personal conditions, or both. It follows that we should make diligent search for all localizing circumstances and remove them, "so as to render the locality untenable for the epidemic." Quarantine leaves all such conditions "untouched and unthought of."

The real question however is, can it prevent the extension of epidemic diseases, whether contagious or not? "If it can it is valuable beyond price; if it cannot, it is a barbarous encumbrance, interrupting commerce, obstructing international intercourse, perilling life and wasting public money." Whether it can do this or not is a mere question of evidence, and everything in India and Britain affirms that it cannot do so.

Professor Caldwell of America says: "Cholera, though a fatal scourge to the world, will, through the wise, beneficent dispensation under which we live, be productive of consequences favourable alike to science and humanity. Besides being instrumental in throwing much light on the practice of physic, it will prove highly influential in extinguishing the belief in pestilential contagion, and bringing into disrepute the quarantine establishments that have hitherto existed."

Measures of prevention and quarantine have been the subject of international conferences held at Constantinople in 1866, Vienna in 1874, and Rome in 1885.

The theories on which the measures recommended by these conferences are grounded have undergone little change since the conference at Constantinople in 1866; the basis on which all the conclusions with regard to preventive measures are built is still, as it was then, the theory of contagion.

Quarantine has, however, gradually been reduced from ten days imposed at the Constantinople conference, to seven days at Vienna, and to five days suggested in the unfinished conference at Rome; and even five days are not to be exacted unless the ship has had cholera on board, or has been gravely suspected, after leaving port. But great stress is still laid on quarantine in the Red Sea, as though that were the channel by which cholera entered Europe, of which there is really no evidence.

Great modifications were suggested at Rome with regard to pilgrim traffic to Mecca, ten days' detention in the Red Sea being reduced to five, and twenty-four hours only being imposed on ships with a clean bill of health.

Land quarantine was declared to be useless at the Vienna Conference, and both that and cordons were condemned at the Roman Conference on the ground that they were impracticable.

It will be observed, that though the theory of contagion

still prevails, it has undergone great modifications, suggesting the hope that the time may not be far distant when reliance will be placed upon sanitary measures which alone offer any guarantee for protection, rather than on such barbarous institutions as quarantine.

The question arises, what does it behove each individual of a community to do, as regards himself, his household, his village, town, and country, when cholera menaces, or has actually made its appearance?

Attention should be directed to careful living, careful clothing, and moderation in habits and diet. Avoid depressing influences, fear, over-fatigue, chills, violent alterations of temperature, aperient medicines, especially those of a saline nature, indigestible food, impure water, unripe or over-ripe fruit, and be careful to observe and promptly check any tendency to diarrhœa.

Pay attention to ventilation, to perfect drainage, to absolute purity of water supply, and to prevention of overcrowding, using all influence to secure this throughout village or town. Do not be afraid to attend upon the sick, for no danger is incurred thereby. Disinfect excreta, and thoroughly cleanse effects, houses and rooms.

Avoid quarantine and coercive measures which divert attention from the true sources of safety, summed up in the expression "complete sanitation."

Although much remains to be known about the causation of cholera and its apparent caprices in incidence and diffusion, yet from what experience and observation have taught us we seem to be warranted in stating the following to be facts with reference to the disease.

1. That cholera has been present in India and other countries from the earliest times, and that isolated cases occur in almost all countries.

2. That cholera is always present, not only in certain parts of India, but elsewhere, and that in India outside these areas its prevalence varies in different years and according to the season of the year.

3. That cholera does not attack all the places within an epidemic area.

4. Meteorological changes produce sudden alterations in the activity and intensity of an outbreak.

5. That the rate and direction of an epidemic are not influenced by facilities of communication or by the greatest streams of human traffic, the opening of the Red Sea route, *e.g.*, not having increased its diffusion.

6. That the cases are more frequent and more severe at the commencement, than in the continuance of an outbreak.

7. That hygienic measures afford the greatest security, but are not an all-powerful safeguard against cholera; local insanitary conditions and impure water favour its incidence and increase its intensity; that it is important to check all diarrhœa in times of cholera prevalence.

8. That cordons and quarantine have utterly failed to prevent the spread of cholera, but on the contrary, have done harm.

9. That to enter an area over which cholera is present, or to travel within that area, is especially dangerous to a new-comer, while residents whose circumstances of living are favourable, have a better chance of escape.

10. That removal is the best course when cholera attacks a regiment or other body of men.

11. That attendants on the sick have not suffered more than others.

12. That impure water, irritating articles of diet, unripe fruit, saline aperients are liable, during cholera prevalence, to bring on diarrhœa and the disease.

13. That fatigue, exhaustion, fear and anxiety are powerful predisposing causes.

14. Many circumstances attending the outbreak of the disease and the pathological conditions then developed, seem opposed to a specific poison as being the cause of the disease.

15. Having suffered from cholera gives no immunity from recurrence of the disease.

The sanitary measures recommended by Government, if carried out, are such as may imbue us with confidence, that if cholera appear, we shall be protected against any intensity of prevalence. The more we can perfect the measures now in force—and much can be done towards this, for insanitary houses are still far too numerous everywhere—the more thoroughly our individual and collective support, moral or material, be accorded, the more complete, we may anticipate, will be our immunity from the disease.

Experience in Europe during the recent epidemic, shows how futile coercive measures have been, while the examples of Marseilles, Toulon, Valencia, Palermo, Naples, whose notoriously insanitary conditions have paid their natural penalty, will be a salutary warning as to how cholera may

be intensified by local causes, and give a lesson which, it is to be hoped, will not be disregarded.

In the *Times* of Monday, February the 22nd, 1886, it was recorded that a memorial to the Lieutenant-Governor of Bengal, concerning sanitation, was laid before the Government of Bengal. This memorial states that since 1881, cholera has swept away more than 20,000 people in Calcutta and its suburbs; that in some suburban wards the death-rate has stood at 70 in the 1,000; that during the decade of 1875 to 1884, out of a population of 257,000 in the suburbs, no fewer than half had perished.

The laws of sanitary science are understood both here and in India, and the enactments of the Government would be effective if fully carried out, but no Government can force sanitation upon towns, villages, or houses, without the co-operation and support of the residents, and all measures will be found useless, unless backed by the personal efforts and exertions of individuals. Experience shows that in this country in the present day the best houses are often most defective, and that local causes of disease, which might be removed, abound, notwithstanding all that is done by the Government Local Board. In India, the reports of the Health Officer of Calcutta show that much is still wanted in that centre of cholera in the way of municipal aid, towards giving full effect to the sanitary measures necessary to control the disease. Let us hope that his advice will be attended to, for surely it would have the best results.

The cholera, which has been in Europe for the last five years, has now apparently died out, or at all events is dormant; but it may appear again, and wherever it can find a fitting nidus, *i.e.*, the presence of bad local conditions, all the quarantine and inspection in the world will not keep it out; that such bad local conditions in towns, streets, and houses exist, is proved by the reports of the Sanitary Associations, and of sanitary engineers who deal with these matters in localities where government officials can exercise no interference. The measures for their removal are simple enough if only the public can be brought to believe in the unseen but removable dangers which exist within, around and beneath their houses.

This is a great sanitary defect of the present day and cries loudly for reform; upon this it may depend whether pestilence shall find footing, or shall leave the locality unscathed.

But I must now bring these remarks to a close. Imperfect and incomplete as the account has been, I trust it may not have altogether failed in shewing how much epidemic cholera is under our own control. That, whatever may be its origin, its incidence, its prevalence and its dissemination are subject to physical laws which, if duly observed and enforced, will protect us from that which, if uncontrolled by the exercise of the sense God has given us, may prove like the destroying angel of the Apocalypse. Happily we have acquired some knowledge of these laws, and it depends on ourselves and how we apply it as to what the results may be. Epidemics are not a necessary, though a constant condition of man's existence on earth. They are amenable to the laws of hygiene and of common sense. "Let us," says Dr. Dallinger in a recent address, respecting small-pox, "do our duty and act up to our knowledge, and as surely as disease comes among a people by physical laws broken, so it will depart from them if they see to it that physical laws are obeyed."

